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SIR HUMPHRY DAVY

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THE SCIENTIFIC ACHIEVEMENTS OF SIR HUMPHRY DAVY

BY

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B.Sc., F.I.C.

OXFORD UNIVERSITY PRESS

LONDON: HUMPHREY MILFORD

1930

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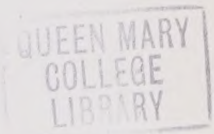

OXFORD UNIVERSITY PRESS
AMEN HOUSE, E.C. 4
LONDON EDINBURGH GLASGOW
LEIPZIG NEW YORK TORONTO
MELBOURNE CAPETOWN BOMBAY
CALCUTTA MADRAS SHANGHAI
HUMPHREY MILFORD
PUBLISHER TO THE
UNIVERSITY

PRINTED IN GREAT BRITAIN AT THE UNIVERSITY PRESS, OXFORD
BY JOHN JOHNSON, PRINTER TO THE UNIVERSITY

PREFACE

SIR HUMPHRY DAVY'S own writings describe his discoveries and scientific conceptions. His younger brother, Dr. John Davy, assembled them in eight volumes, and they were published under the title *The Collected Works of Sir Humphry Davy, Edited by his Brother, John Davy, M.D., F.R.S.*, in 1839–40. These writings, so conveniently assembled by his brother, constitute the chief source for a record of Sir Humphry Davy's scientific achievements. A single biographical volume preceded the volumes 2 to 9 that contained the assembled writings.

Dr. Paris published his *Life of Sir Humphry Davy*, in two volumes, in 1831. Dr. John Davy published his *Memoirs of the Life of Sir Humphry Davy*, also in two volumes, in 1836. His preliminary biographical volume of the *Collected Works* was based on this.

Dr. John Davy was seriously disturbed and greatly annoyed by the 'nature of Dr. Paris's work'. It was just to Sir Humphry Davy 'as a man of science, and an original inquirer', but it sullied his reputation 'as a man and a philosopher'. He therefore carefully corrected the depreciatory items of Dr. Paris's biography. The modern reader will smile occasionally over Dr. John Davy's solemn corrections, and perhaps suspect him of having a small sense of humour. He seems, for example, to have been needlessly

annoyed by the account of the syringe presented to the young Humphry Davy by the 'ship-wrecked French surgeon'. According to Dr. Paris this 'long-neglected and unobtrusive machine', before an hour had passed, appeared 'in all the pomp and glory of a complicated piece of pneumatic apparatus'. He was suggesting that Sir Humphry Davy was helped to his 'wonderful tact of manipulation', fertility in expedient, and power 'of contriving apparatus' by his early shifts with available material. Dr. John Davy somewhat unnecessarily suspected 'some facetious person' behind an invented story, and somewhat undiscerningly complimented Dr. Paris's pompous paragraph on its 'minuteness and vivacity worthy of De Foe'. Dr. John Davy may also be suspected of too excessive a seriousness over the reference to the 'Tepidarian Society'. The Tepidarians were so named because they only drank tea at their meetings. According to Dr. Paris they helped Humphry Davy to a quicker popularity by gathering his first audiences. It was hardly necessary in 1836, when insisting on Humphry Davy's 'rapid and brilliant success at the Institution', for Dr. John Davy to take the Tepidarians too seriously. No society, however faithful to tea or loyal to its member, could do for Humphry Davy what his own talents and eloquence did. The biographical brother was, however, well qualified by intimacy and close fraternal relations to write the memoirs of Sir Humphry Davy. The historian, if he is discriminat-

ing, can welcome the two different aspects in which the two biographies often present their subject. He can remember that if Dr. Paris was on occasion too uncomplimentary, Dr. John Davy was ever the admiring brother. The biographical scuffles are, in any event, less important for a work that, like the present volume, is devoted primarily to the scientific achievements of Sir Humphry Davy. Both biographers were well qualified by their scientific knowledge to discuss these.

Sir Humphry Davy can also be seen through other eyes of his times in the pages of the *Edinburgh Review*, the *Quarterly Review*, the *Philosophical Magazine*, Nicholson's *Journal*, the *Gentleman's Magazine*, and other periodicals. Dr. Thomas Thomson described his work in a *History of Chemistry*, 1830–1. This contemporary record appeared just after Davy's death in 1829. Sir Humphry Davy can also be seen through Faraday's eyes in *The Life and Letters of Faraday*, by Dr. Bence Jones. Faraday, who was Davy's assistant for some time and closely associated with many of his researches, knew him too well to be either blind to his faults or ignorant of his scientific genius. The record can be completed by many references scattered through many pages. These sources have been consulted in preparing the present volume.

The centenary of Davy's death in 1929 is an appropriate occasion for reminding the world of his scientific achievements. Each generation requires

even its own history of the past. No history can be quite final, for even if the past does not change, the eyes that perceive it do. Sir Humphry Davy's achievements and ideas may be the same now as they were in 1829, but our eyes are not the eyes either of Dr. Paris or Dr. John Davy. The longer retrospect differs from the shorter. The brevity commended to the historian by Lucian is also commended by historical remoteness. Most modern readers want less than Dr. Paris or Dr. John Davy more acceptably gave. The present work also confines its record more exclusively than his two more immediate historians to the science of Sir Humphry Davy. It employs only sufficient biographical detail to provide the necessary framework of the narrative. Sir Humphry Davy is not a permanent personage of history because he had human frailties, nor because of the incidents of his life, nor even because he drew great audiences. His significance is a scientific significance. An effective summary of his discoveries and theories provides one very comprehensive view of the movement and progress of science during his lifetime. This summary and view constitute the theme of this book. If the effort of the volume fails, the writer, and not the theme, will be at fault.

The present volume concentrates more exclusively on Davy's science than Thorpe's more recent *Humphry Davy, Poet and Philosopher*. A few of the topics discussed perhaps merit a special notice here. It seems highly probable that Prout received a hint

at his familiar hypothesis from Davy. The possibility of a revised phlogiston theory was seriously reviewed by Davy during a period of perplexity. This interesting index to the power of phlogiston is perhaps less familiar than it deserves to be. Davy's debt to Dalton is not always sufficiently recognized. There is a stress in the text on the aid rendered to Davy by Dalton's 'proportions'. Sir Humphry Davy also anticipated many changes of scientific opinion. These included, as is duly noted, the hydrogen theory of acids, a mechanically mixed instead of a chemically combined air, and the final English adoption of H_2O for the formula of water.

J. C. G.

BRADFORD, YORKSHIRE,
31 *March* 1930.

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I

PHOSOXYGEN

CANDOUR imposes one unpleasantness upon the recorder of Sir Humphry Davy's scientific achievements. A preliminary paragraph on the unfortunate side of his character will avoid a further troubling of the record with this unpleasantness. Faraday, who was assistant to Davy for some years, and closely associated with him for a period, said that he used his master as a model of what to avoid. Davy was spoiled by success. He was greedy for fame, and when fame came very quickly it quickened his egotism. It was bad for Davy the man, though it may have been good for Davy the natural philosopher, to become a famous discoverer and a public favourite before he was 24. He disliked being excelled in his favourite sport of angling, and he resented any suggestion of challenge to his own scientific supremacy. His egotism, present from the first and fed by success, often made him tactless towards his fellows or irritable towards his critics. In one unfortunate episode it made him jealous enough of Faraday to oppose his election to the Fellowship of the Royal Society. It made him often haughty or overbearing, and it made him sometimes ungenerous to those whom he regarded as rivals. His close brotherly relations with Dr. John Davy were probably promoted, to say it without cynicism,

by the unswerving admiration of the younger. Sir Humphry Davy was not all deficiency as a man, nor only great as a natural philosopher. He was at his best when he devoted his scientific talents to inventing a lamp to make coal-miners safe without patenting it for profit. There must be candour enough in the record, however, to admit that Davy the man was much less noble than Davy the natural philosopher was great. His married life was marred by an incompatibility of temperament, though Lady Davy's presence in 1829, when he went to Geneva to die, shows that it was not entirely destroyed. The deficiencies of the man must be noted, and they can be the more candidly admitted by the most reluctant recorder because they are so compensated or dimmed by the splendour of the scientific achievement. Faraday knew well that Davy the natural philosopher was genuinely great, though he derived many warnings from deficiencies of Davy the man. There have been nobler men than Davy, there may have been greater than he in science, but he has never been excelled in his combination of genuine scientific achievement with high contemporary public favour. Sir Humphry Davy is almost, if not quite, unique as a truly great natural philosopher who also secured a fervent popular acclaim.

If Dr. Johnson were alive now he would probably say that Davy was made into a natural philosopher, or into a chemist, by reading Nicholson's *Dictionary of Chemistry* and Kerr's translation of Lavoisier's

Traité Élémentaire de Chimie. 'The true genius', according to the doctor, 'is a mind of large general powers, *accidentally* determined to some particular direction.' The genius, on this version, is ready for all things, but chosen by some circumstance for one, as Sir Joshua Reynolds became a painter by reading Richardson's treatise on painting. Spenser's *Fairy Queen*, Dr. Johnson said, made the young Cowley feel the charms of verse, produced in him a definite propensity, and left him 'irrecoverably a poet'. Dr. Johnson believed that a *precipitating occasion* decided direction of achievement, as the *Fairy Queen* made Cowley a poet, or Richardson's treatise made Reynolds a painter. He would presumably have said, if he had lived after Sir Humphry Davy instead of before him, that Nicholson's *Dictionary* and Lavoisier's treatise made the young Davy irrevocably into a chemist.

Humphry Davy was born at Penzance on December 17, 1778. He was therefore nearly nineteen years old when he 'began the study of chemistry in November or December' of 1797. The famous treatise of Lavoisier and the portly dictionary of Nicholson were the precipitating occasion of the devotion to chemistry that soon became the predominant purpose of his life. Dr. John Davy, who was always closely friendly with his brother, ever his admirer, and finally his biographer, noted various other predisposing circumstances that helped Humphry into scientific interests. The rocks of

the Land's End or Lizard, for example, excited in him an interest in 'the mysterious operations of nature'; the wave-thrown or vegetating weeds by the sea were actually the objects of some of his first original researches; the 'surrounding mines of copper or tin' were also an attraction and a stimulus. The biographical brother also stressed the intercourse between the young Humphry and Mr. Gregory Watt and Mr. Davies Gilbert. The treatise and the dictionary, however, so far as any single circumstance can be justifiably selected, precipitated the young Davy into the great work of his life. He had been apprenticed to Bingham Borlase, a practising surgeon and apothecary in Penzance, early in 1795. Professional interests may therefore have directed him to the two authors who quickly transformed him from a physician into a chemist.

It is doubtful whether this precipitating occasion *made* him a chemist, or determined him into natural philosophy, as Johnson would probably have said. Most men probably *respond* to their precipitating occasions because their inclinations or talents predispose them to respond. Cowley may at least be suspected of becoming a poet through Spenser's *Fairy Queen* because he was poetically inclined. Lavoisier and Nicholson probably influenced a naturally responsive Humphry Davy. Coleridge thought that Davy would have become pre-eminent as a poet if he had not been pre-eminent among 'philosophers and scientific benefactors'. If

Dr. Johnson had been contemporary with Davy he might have added a regret to this verdict. The 'knowledge of external nature' did not seem to Johnson to be 'the great or frequent business of the human mind'. Many comments scattered through Johnson's writings reveal that he preferred one poet to many natural philosophers. He would almost certainly have regretted that the *Fairy Queen*, or an equivalent, did not make Humphry Davy into a poet before Lavoisier and Nicholson made him into a chemist. Davy did write poetry, and he wrote it till the end, but it is at least highly doubtful whether Davy the poet were ever a serious rival or alternative to Davy the natural philosopher.

Sir Humphry seems to have had the supreme pleasure and great success of great powers exercised precisely where they were most able. Nature seems to have equipped him chiefly with just those talents that he exercised in his discoveries, and to have filled him chiefly with just those desires that he gratified by his researches. His quick eager response to chemical interests is unmistakable. A few months after he began to study chemistry in late 1797 he was writing his *Essays on Heat and Light, &c.* They were published in 1799, less than two years after his chemical studies began, to commend new versions of some phenomena to science, and they temerariously included an attempt to amend Lavoisier's theory of combustion. They contain crude speculations and imperfect experiments, but they clearly display a

young investigator joyfully dropping into the very niche for which nature had intended him. It was no mere spasm of momentary enthusiasm touched off by Lavoisier and Nicholson. The laboratory was too obviously his most genuine home, and scientific research too obviously his most genuine interest, for Humphry Davy the natural philosopher not to outpace Humphry Davy the poet. 'Experimenting' was avowedly his 'favourite pursuit' in 1800, and his 'real' existence three years after was equally avowedly 'amongst the objects of scientific research'. He is discovering still, Faraday wrote of him some twelve years later, as is his practice. His brother John, whose uniform admiration did not blind him here, insisted on his 'passion for research'. Davy the philosopher was also the more genuine Davy than Davy the poet in his comparisons of poetry with science. In 1805, when he was about 27, he confided to his note-book that while poetry amuses more than it instructs, science instructs more than it amuses. He also insisted on the universally intelligible language of facts as the prerogative of science. Davy could not live without a laboratory. Even in 1812, when he was just married, a 'portable chemical apparatus' accompanied his 'tour of pleasure' with his wife through 'the Highlands of Scotland'. Continental laboratories were to be open to *Sir* Humphry Davy, as he became in 1812, when he travelled on the Continent with his wife in 1813, but the 'portable chemical apparatus' travelled with him too, for research

was always to be possible. Faraday also went with him as assistant. Nothing could stop Davy's march of discovery, nor divert him from it. Neither social pleasures nor social prestige could wean him from the laboratory. There is little sign that when social allurements made Davy's friends anxious for him they stayed the march of his discoveries. Social prestige did somewhat spoil the man, but it did not spoil the chemist. Scientific discovery drew him too effectively to permit a return to medicine, or to the fortune and fame of a great physician. He did think of returning, he even prepared to return in 1810, but his natural bent restrained him. Nor could the Church persuade him to devote his eloquence to religion. Romance could no more effectively divert him from science than could social prestige or medicine or the Church. Only a specially charming woman, he wrote to his mother shortly before his marriage, could have brought the interruption of courtship, or the final issue of marriage, into his life of research. When he did marry Mrs. Apreece, in 1812, the tenor of his life, as his biographical brother noted, remained conformed to 'habits of research': these were only disturbed so far as marriage did inevitably disturb them. Sir Humphry Davy's whole career suggests that nature intended him to be a natural philosopher by endowing him with high appropriate powers and with intense appropriate desires. When Lavoisier and Nicholson had precipitated him into chemistry, a natural philosopher

he became and a natural philosopher he remained. Nothing could divert him from research: neither fame, nor social prestige, nor medicine, nor religion, nor love, nor death itself until it struck its final blow, for Davy was inquiring at the last. Davy was born to be a chemist; Lavoisier and Nicholson merely revealed his destiny to him.

Dr. John Davy, the admiring brother and biographer, somewhat naively remarked of Sir Humphry that 'he always had a wonderful facility in relinquishing an opinion'. Sir Humphry himself gave another description of his aptitude: he was not too much 'devoted to consistency'. It was characteristic of Sir Humphry Davy to anticipate changes of opinion by changing his own, and he very frequently provided the discoveries that made opinions alter. He was no weathercock to blow with any wind, nor was he without settled convictions, nor, perhaps, without some pet ideas, but he could test preconceived opinions severely or surrender them to facts. It was thus characteristic of the young Davy, yet immature and unpractised in research, to make a youthful effort to modify Lavoisier's theory of combustion.

Though the French revolutionaries had guillotined Lavoisier in 1794, they had not guillotined his concepts. When Lavoisier's *Traité Élémentaire de Chimie* was published in 1789 Kerr had promptly translated it into English. This translation was published in 1790, and another edition appeared in 1796. Lavoisier had steadily revolutionized chemistry from

November 1, 1772, when he deposited his famous note with the French Academy, to 1789, when he published his famous treatise. The phlogiston theory had been steadily succumbing to Lavoisier's experiments and expositions when the treatise appeared, and the treatise itself helped many chemists to decide for the new chemistry. Though Stahl, the great protagonist of the phlogiston theory, was a German, the younger German chemists steadily became antiphlogistians. In 1792 Klaproth repeated 'all the requisite experiments' of Lavoisier before the Academy of Sciences of Berlin. 'The result', Dr. Thomas Thomson added in his *History of Chemistry* in 1830–1, 'was a full conviction on the part of Klaproth and the Academy, that the Lavoisierian theory was the true one.' The French chemists had been steadily acceding to Lavoisier, and the English chemists were steadily acceding too. There were recalcitrants, however, and, though Nicholson's *Dictionary of Chemistry* did not appear till 1795, Dr. John Davy, writing in 1836, could call it 'a kind of border tract between the old and new doctrines'. Dr. Joseph Priestley died in 1804 an unconverted phlogistian, and when the Honourable Henry Cavendish died in 1810 he had not publicly disavowed Stahl, nor does any record show that he had acceded to Lavoisier. When the young Davy began to study chemistry in the late 1797 he would study the prevailing version of combustion, with its implications or consequences, in Lavoisier's treatise.

Though the rejection of phlogiston seemed to Nicholson to simplify chemistry, the young Davy would also realize through the *Dictionary* that the older notions lingered on. Davy himself adopted the antiphlogistic chemistry of Lavoisier, though he modified many details, but at one period of his life, when he was struggling for comprehension, he did review the possibility of a revised phlogiston theory. He never actually reverted to the phlogistic version, old or revised, as his brother rightly insisted, but his free spirit of inquiry did make him vigilant against even the concepts so apparently established by Lavoisier.

When quicksilver is gently boiled in the presence of air a red powder gradually covers it. A black solid forms when copper is heated in air, and many other metals also form variously coloured products when they are similarly heated in the presence of air. Such a product of a metal heated in the air was called a 'calx', and the term 'calxes', or 'calces', was used in the plural. These calces are the modern 'oxides'. Stahl, 1660–1734, explained these events by

$$\text{METAL} \quad (\text{Calx} + \text{phlogiston}) = \text{calx} + \text{phlogiston}.$$

The phlogiston escaped when the metal was heated, and the calx remained. The calx was *heavier* than the original metal, though phlogiston was presumed to have escaped. Stahl ignored this, but many chemists tried to explain it, and one method of explanation is specially relevant here. The metal was supposed to

be (X+phlogiston), where X was not the calx. When the phlogiston escaped during heating the X united with *something* to form the calx. The process was

$$\underset{(X+\text{phlogiston})+\text{something}}{\text{METAL}} = \underset{(X+\text{something})+\text{phlogiston.}}{\text{CALX}}$$

The calx was heavier than the metal because the added *something* was heavier than the fugitive phlogiston. The *something* varied with the fancy of the phlogistian, but this type of explanation had an important development when Lavoisier steadily convinced chemists, after about 1774, that the *something* was *oxygen*.

Lavoisier also steadily established a second important point. The total weight of the calx, the *oxyd*, as it was gradually renamed, exactly equalled the combined weights of the original metal and the combining oxygen. This did not agree with

$$\underset{(X+\text{phlogiston})+\text{oxygen}}{\text{METAL}} = \underset{(X+\text{oxygen})+\text{phlogiston,}}{\text{CALX}}$$

but it did agree with

$$\text{metal}+\text{oxygen} = \underset{(\text{metal}+\text{oxygen}).}{\text{CALX}}$$

The phlogistians could reply by supposing

$$\underset{(X+\text{phlogiston})+\text{oxygen}}{\text{METAL}} = X+(\text{phlogiston}+\text{oxygen}) = \underset{(X+(\text{phlogiston}+\text{oxygen})).}{\text{CALX}}$$

The phlogiston escaped, united with the oxygen, and

then the compound of phlogiston with oxygen united with the residue *X*. The two explanations were now the same in supposing the metal to unite with oxygen during the formation of the calx, but, while Lavoisier supposed the metal to be a single elementary body, the phlogistian supposed it to be a compound of *X* and phlogiston. In the equation representing Lavoisier's version, calx = metal + oxygen, the phlogistian could substitute (*X* + phlogiston) for metal. The discovery of the composition of water by Cavendish in 1781 strengthened this phlogistian exposition by allowing the identification of phlogiston with *hydrogen*, and of the augmenting compound (phlogiston + oxygen) with *water*. The revised phlogistianism of later days, including Davy's own surveys of possibilities, depended essentially on the identification of the old phlogiston with *inflammable air*, or, as it was subsequently called, *hydrogen*.

The young Davy would read in Nicholson's *Dictionary of Chemistry* that both the phlogistic and antiphlogistic theories recognized the analogy between the action of acids upon metals and the action of heat 'with access of vital air' (oxygen) upon them. On the phlogistic version, for example, zinc was (*X* + phlogiston). When dilute sulphuric acid acted on the metal it united with the *X*, and the phlogiston escaped. The acid of solution obviously corresponded to the *something* of calcination. Cavendish collected the *inflammable air*, or gas, from the reaction

in 1766, and suggested that this might be the phlogiston. Lavoisier subsequently called this gas *hydrogen*. Cavendish himself discovered in 1781 that the *inflammable air from metals*, or hydrogen, otherwise called phlogiston, united with oxygen to form *water*, and by 1783 Lavoisier had fully confirmed this.

Water was very generally admitted to be a compound of oxygen and inflammable air after 1783. Lavoisier called the inflammable air 'hydrogen'; phlogistians usually called it 'phlogiston'. Phlogistians identified iron with (X + phlogiston); Lavoisier regarded it as an element. Dilute sulphuric acid, according to phlogistians, united with the X, and the phlogiston escaped. When iron was heated in air its phlogiston also escaped. The fugitive combined with the oxygen of the air to form water that then united with the residual X. Lavoisier preserved the parallel between solution in acid and calcination differently. Heated iron combined with oxygen to form a calx, or oxyd. In dilute sulphuric acid it formed the calx by acquiring oxygen from the water. The hydrogen escaped from the decomposed water, and the oxyd combined with the acid. The reaction was

WATER OXYD
(hydrogen+oxygen)+metal = (metal+oxygen)
+hydrogen,

followed by union between the oxyd and the acid.

The phlogistian reaction

METAL CALX
(X+phlogiston)+oxygen = (X+phlogiston+oxygen),

finally resulting from the emission of phlogiston, its union with oxygen, and the combination of the consequent water with *X*, was simplified by Lavoisier into

metal+oxygen = (oxygen+metal).

This simplified version steadily prevailed, and it was prevailing when Davy began to study chemistry in the late 1797. Davy did not question this, but he did question Lavoisier's supplement to it.

A thin iron spiral burned in oxygen with evolution of heat and light. A union between metal and oxygen explained the oxyd, but it did not explain the heat and light. Lavoisier's *theory* of combustion or calcination suggested an explanation. Oxygen *gas* was a compound of oxygen *base* and *caloric*, or heat. Lavoisier also suggested, though with even less assurance, that *light*, as well as *caloric*, was combined with oxygen *base* in oxygen gas. Neither caloric nor light affected the balance, though the oxygen base could be weighed. When iron burned the process was

OXYGEN GAS
iron+(oxygen base+caloric+light) =
OXYD
(iron+oxygen base)+caloric+light.

Both ponderables, substances that could be weighed, and imponderables, substances that could not be weighed, thus participated in the process. The ponderable part was responsible for the equality of the combined weights of the participating oxygen

and metal with the weight of the oxyd. This has survived, though chemists, who do not now recognize caloric, speak only of oxygen, and not of oxygen base. The imponderable part was responsible for the heat and light that were due to the emissions of presumptive caloric and presumptive light respectively. *Presumptive*—Lavoisier warned chemists that his exposition was provisional. The emission of caloric was less perceptible during more quiet calcination, as when mercury slowly formed a red oxyd, and the emission of light was still less perceptible.

The flaming combustion of brimstone or oil, or of other bodies, was for phlogistians essentially an escape of phlogiston from the dismembering body, and its further adventures. It was essentially combination with oxygen base for Lavoisier, and the evolution of heat and light was again the emission of caloric and light from the decomposed oxygen gas. When charcoal burned the process was

OXYGEN GAS
carbon+(oxygen base+caloric+light) =
CARBONIC ACID
(carbon+oxygen base)+caloric+light.

Hydrogen *gas* contained hydrogen *base*, caloric, and light, analogously to oxygen gas. When these two gases united with vigour to produce water, most of their caloric and most of their light was emitted in the fuss.

The relations of caloric to substances were more amplified in Lavoisier's theory of combustion and

calcination than in this summary, but this summary, though perhaps drastic, is sufficient for understanding and for present purposes.

Since Lavoisier himself more emphasized presumptive caloric than presumptive light in oxygen gas, there was perhaps some excuse for the emphasis in Nicholson's dictionary, and elsewhere, on caloric-oxygen, or caloric-oxygen base. The significance of light may have directed the young Davy's first crude experiments, and pervaded his speculations, without hints from Lavoisier. The cosmic scope of light was a very familiar theme. Lavoisier had surmised a hint at a great philosophical truth in the 'fable of Prometheus'. Nature would be 'lifeless and inanimate' without the light that the 'benevolence of Deity' had given to furnish the earth 'with organization, sensation, and intelligence'. Davy perceived great reservoirs of light in the 'sun and the fixed stars'. The 'great Organizer' had destined them 'to diffuse over the universe organization and animation'. This passage may be a natural parallel to Lavoisier's without being a copy, but Davy had forgotten both the prototype of his paean, and Lavoisier's explicit connexions of light with oxygen, when he accused him of the 'total neglect of light'.

The young Humphry Davy, in his prompt speculative effort, noted two defects in the 'theory of the immortal Lavoisier and the ingenious French nomenclators': supposititious caloric, and light disregarded. He therefore amended it doubly by substituting

phosoxygen for caloric-oxygen, and repulsive motions in particles, as he called them, for imaginary caloric.

Phosoxygen, ordinary oxygen gas conceived as a compound of oxygen and light, was Davy's substitute for the repudiated caloric-oxygen. When phosphorus, or sulphur, or carbon burned in oxygen gas, phosoxygen, the combustible united with the oxygen, and released the light. Davy assumed that his own

$$\text{combustible} + \underbrace{(\text{oxygen} + \text{light})}_{\text{OXYGEN GAS}} = \underbrace{(\text{combustible} + \text{oxygen}) + \text{light}}_{\text{COMBUSTED PRODUCTS}},$$

replaced Lavoisier's

$$\text{combustible} + \underbrace{(\text{oxygen} + \text{caloric})}_{\text{OXYGEN GAS}} = \underbrace{(\text{combustible} + \text{oxygen}) + \text{caloric}}_{\text{COMBUSTED PRODUCTS}}.$$

He even detected a loss of weight through the escape of light. The combustibles were enclosed in a glass globe filled with phosoxygen (oxygen gas), and 'fired by a burning-glass'. The weight of the system seemed to be less after combustion than before it, because light had escaped from it. His balance, he admitted, was not very sensitive, but it discovered a marked 'deficiency of weight' when phosphorus burned to phosphoric acid in phosoxygen, and smaller diminutions when carbon or sulphur were used.

Pure 'oxyd of lead', continued the young and in-

experienced experimenter, contained lead and *oxygen*. Light could remove the oxygen to form phosoxygen, and leave the lead. No heat altered it, Davy affirmed, if light were excluded. If light were directed on to it, more generously by a burning-glass, or even more mildly by the beam of a candle, the oxyd separated into lead and phosoxygen (oxygen gas). The *oxygen* of the oxyd required light, or more light, to form phosoxygen, was refused it in the dark, and received it sufficiently even from a candle.

The impetuous inexperience of Davy is very evident in the glaring errors about minium. Nicholson had stated in his *Dictionary* that light made 'metallic calces' 'nearer the state of combustibility than before'. Davy may have caught at this clue too eagerly, or have been too predisposed by his own notions, to observe very carefully. The 'deficiency of weight' in his enclosed combustions was probably doubly due to a disposition to discover it and to an immature experimental technique.

Mercury seemed to Davy to combine directly with phosoxygen, with both the oxygen and light of oxygen gas, for no light was visibly emitted during the combination. The resulting 'mercuric phosoxyd' was the traditional '*red* oxyd of mercury'. 'Plumbic phosoxyd', that also contained oxygen and *light*, was also red. The 'yellow oxyd of tungsten', that seemed also to be a 'phosoxyd', became *blue* under solar light. The colours of bodies seemed to be connected with their content of oxygen and light, and to vary with

the proportions of these. Davy was not frugal in exposition: the negro would be white if his skin retained oxygen, for oxygen alone conferred whiteness, but light removed it (to form phosoxygen), and then predominant carbon made him black. Women, added the comprehensive theorist, are fairer than men because they are less in the light: he could not have said this to-day.

Lavoisier had surmised, from Berthollet's experiments, that combined light assisted the caloric to maintain oxygen gas in the gaseous state. He also hinted at combined light in vegetation, and at its possible connexion with the green leaves or the colours of flowers. Davy had forgotten this when he accused the French chemist of neglecting light, if he had ever read it in the treatise, but he developed these notions very profusely in his speculative rush. If light and oxygen combined in various proportions, a 'luminated phosoxygen', an oxygen gas containing a large proportion of combined light, might stretch through the upper heavens. If phosoxygen united totally with venous blood in the lungs, as experiments seemed to show, if vegetation displayed colours under solar rays, and the hues of animals were connected with exposure to the sun, natural colours depended on combined oxygen and light.

The combustive dispersal of the light combined in phosoxygen required another exposition. The attractive forces characteristic of matter, Davy surmised, united particles of light to particles of 'oxygen'. The

light was condensed into the oxygen gas, or phosoxygen, so that its particles were in 'a state of contiguity', because the particles of 'oxygen' attracted the particles of light. When the light was detached from the 'oxygen' during combustion the 'great repulsive motion acting on the corpuscles of light', which greatly exceeded most attractive forces acting upon them, and was eminently characteristic of this substance, could throw it into 'a state of existence' peculiar to itself. When the 'amazingly minute' particles of light streamed through space, with an 'almost inconceivable velocity', and almost completely defying gravitation, they were subject to a 'continual separating motion'. The fugitive light from combustion was usually in this state of *repulsive projection*, as Davy called it, and as others were to call it after him. This notion, and others similar to it, played a considerable part in speculation, and continued to play it after Davy's youthful excursus. The particles of light, on this version, violently repelled one another. They were compelled to pack closely on occasion, as in phosoxygen, because other particles attracted them. When they were released they were abandoned to their mutual, and powerful, repulsions, and they then streamed violently in *repulsive projection*, being projected by their mutual repulsions. Davy also surmised that the condensed form of light, in which its particles were approximated or packed together, constituted the 'electric fluid'. The flash of the electric spark constantly

compelled presumed connexions between light and electricity. Corpuscles of light packed together into the 'electric fluid' and violently projected in actual light were not confined to the rapid speculations of the young Humphry Davy.

Though Davy did not realize it, his phosoxygen was Lavoisier's oxygen gas with caloric left out. He explained the emission of light during combustion as Lavoisier did—by the ejection of light from oxygen gas. He was less cautious, however, than Lavoisier in presuming either light in oxygen gas or light combined in other bodies. Lavoisier also tentatively explained the heat of combustion by the ejection of caloric from oxygen gas, but Davy's phosoxygen, since it contained no caloric, could not eject it. Davy looked elsewhere for the emission of the heat.

The caloric theory had a rival, and Davy chose it. Heat was often supposed to be a weightless elastic fluid spread like water through a sponge in substances, and at least partly combined with them. This popular theory became still more popular, and still more authoritative, when Lavoisier tentatively adopted it. The presumed fluid was also permanently named 'caloric' in English when Lavoisier christened it 'calorique'. There had been a rival theory, however, since the seventeenth century at least, if not before, that was still a rival, though it was weaker than the caloric version for the moment. Lavoisier lodged caloric in oxygen gas to be ejected

as perceptible heat during combustion, but Davy, discarding caloric, chose the rival theory.

The Right Honourable Robert Boyle, 1627–91, watched an artificer boring a cannon, and saw the shavings fall. He saw an onlooker, when asked to pick up these shavings, snatch his finger from the *hot* metal. The shavings became hot, Boyle reflected, because the friction of the twirling bore had thrown their internal particles into more violent tumultuous motions. 'Intestine' motions, internal movements of particles within bodies, was the rival version to caloric. 'Two opinions have long divided the scientific world. One is that heat consists of a peculiar motion or vibration of parts of bodies, so that the temperature is higher the stronger the vibration. The other is, that heat is a substance or fluid, whose greater or less quantity produces a higher or lower temperature.' Davy would probably read this passage in Nicholson's *Dictionary of Chemistry*, for it was there to read.

Brass guns were convincing Count Rumford that bodies became hotter because internal motions of their constituent particles became quicker. Continuous boring steadily heated them and threw out hot chips. The motion of the bore passed into the motions of the particles within the cannon. The young Davy, between late 1797 and early 1799, was rubbing 'two parallelopipedons of ice' into water, and wax into a liquid. Davy rubbed by a mechanical arrangement within an exhausted receiver. He also

placed ice round the wax to prevent caloric, if it did exist, from flowing into the wax. He concluded from his experiments, as Rumford did from his, and as Boyle had deduced from the bored gun, that heat was, as he called it, 'repulsive motion' of particles. The vigour of combustion, as when phosphorus burned in oxygen, excited the particles of the participating substances into more violent motions, and thus made the bodies hotter. Lavoisier identified the heat of combustion with emitted caloric; Davy identified it with a more violent motion of particles in the participating substances.

Davy did not attribute the heat of combustion *only* to a lash of the participating particles into more violent motions. He supplemented this version by another explanation that was then in vogue. A pound of water requires more heat to raise its temperature through, say, twenty degrees of temperature than one pound of mercury: its *capacity for heat* is greater. If the same amounts of heat are added to a pound of water and a pound of mercury respectively, the water and mercury being at the same initial room temperature, the mercury will become much hotter, attain a higher temperature, than the water, because its capacity for heat is less. Thus, if heating the water also altered it into mercury with a less capacity for heat it would get much hotter than if it remained water. Davy supposed, as many were supposing, that, for example, when phosphorus and oxygen combined during combustion, the product had a less

capacity for heat than the original substances, and would therefore get hotter than it would otherwise do, just as water would get much hotter than it would otherwise do if the heating did convert it into mercury. Water does not become mercury by being heated, and other circumstances might interfere with the inference if it did, but this explanation, with which Davy supplemented his other, was having a vogue. The products of combustion were supposed to have a less capacity for heat than the original substances, or, as Davy expressed it, to have greater 'capabilities for heat', greater capabilities, that is, of becoming hotter, and thus to increase in temperature.

Plants had been observed to emit oxygen under the influence of light. They were suspected of taking hydrogen from water, and liberating the oxygen. Nicholson's *Dictionary* admitted that this contested point was 'far from being disproved'. Davy described experiments on 'marine vegetables', and inferred from them that they did take hydrogen from water. Water contained hydrogen and 'oxygen': the hydrogen went to the plant, and the 'oxygen' united with light to form phosoxygen, or oxygen gas.

Davy also inferred, as many suspected, that plants, including 'marine vegetables', took carbon from carbonic acid, and again liberated oxygen. Carbonic acid contained carbon and 'oxygen': the carbon went to the plant, and the 'oxygen' united with light to form phosoxygen, or oxygen gas. Davy discovered an evolution of oxygen gas, phosoxygen as he called

it, when the plant *arenaria tenuifolia* was exposed to solar light in an atmosphere of carbonic acid. Since plants cannot utilize undiluted carbonic acid, he was as yet an inexperienced investigator. Davy realized that carbonic acid was emitted into the atmosphere by combustion, chemical processes, and by breathing animals. He also rightly realized that plants did decompose the carbonic acid and return oxygen gas to the air, though his experimental verification was imperfect. These facts were suspected then, and known now. It seemed to Davy that the carbonic acid, being heavier than air, would settle like a blanket on the earth's surface if plants did not decompose it. The *diffusion*, or spreading, of the heavier carbonic acid through the lighter air was long to be a puzzle, and Davy quite naturally thought that carbonic acid would try to settle to the bottom of the atmosphere, and stay there if it could, as mercury falls below water and remains.

II

NITROUS OXIDE

GASES were exciting great hopes of medical cures. Dr. Joseph Black finally distinguished 'fixed air' from other 'elastic fluids', now called 'gases', in 1757. Bergman called it the 'aerial acid', because it was weakly acid and spread in the air; Lavoisier called it 'carbonic acid'; it is now called 'carbon

dioxide'. MacBride suspected medicinal powers in this fugitive product of fermentation, and prescribed wort to ease the stomach because this unfermented beer would ferment when taken to produce the beneficial 'fixed air'. Priestley expected great effects from water charged with the gas. There were high hopes when the Admiralty sought a cure for scurvy in Priestley's impregnated water, and unrealized efforts, later on, when experience finally convinced Hey of Leeds that 'fixed air' did not cure 'cancerous ulcers'. Priestley's discovery in 1774 of 'dephlogisticated air', that Lavoisier subsequently called 'oxygen', added other hopes to expectations of pneumatic cures, and Jan Ingenhousz solemnly warned practitioners that oxygen was no Elixir of Life. There were other gases also to excite hopes, and these hopes of the time cancelled Humphry Davy's apprenticeship to Borlase on the first day of October, 1798, to carry him to Clifton, a suburb of Bristol, on the second. Dr. Beddoes had persuaded subscribers to maintain a Pneumatic Institution at Bristol to investigate the medicinal effects of gases. He made Davy the superintendent. He chose well: 'a young man of zeal and genius', Dr. Thomas Thomson remarked in his *History of Chemistry*, 1830–1, was required to 'compensate for the badness of the apparatus and the defects of the arrangements'. A laboratory was ready for the young superintendent, in his house in Dowery Square, Clifton, by the beginning of 1799.

Davy's youthful experiments and speculations had impressed Dr. Beddoes. The correspondence between the two, which soon resulted in Davy's appointment, gave him his first step in his brilliant career of achievement.

These same experiments and speculations also brought him a quick pain. They were collected into *An Essay on Heat, Light, and the Combinations of Light*, and into *An Essay on the Generation of Phosoxygen, or Oxygen Gas, and on the Causes of the Colours of Organic Beings*. Dr. Beddoes published them, early in 1799, in *Contributions to Physical and Medical Knowledge, Principally from the West of England, Collected by Thomas Beddoes, M.D.* Some critics had a heyday of scoffing when the two essays appeared. Their gibes had touched Davy to the quick when he repentantly disclaimed his 'infant chemical speculations' in the December of 1800. He had regretted his sally into science very quickly, for even in the August of 1799, shortly after publication, he was confessing to his note-book that his 'new theory of chemistry' had been published too hastily.

If the 'distinguished and venerable Priestley' was impressed by the author's 'philosophical acumen', Davy may have regretted his impetuous publication too bitterly. The episode, however, left its mark permanently upon Davy. It steadied his impetuosity into a rapid effectiveness. Nicholson's *Dictionary* 'set very little value upon the ingenious conjectures presented to the Public' by many 'men of ability'.

Davy confessed to beginning 'the pursuit of chemistry' with 'speculations and theories'. He had been convinced by 'more mature reflection' of his 'errors', the 'limitation of our powers', and the 'dangers of generalisation'. A rapid maturity transformed him from an inexperienced chemist into an inductive thinker and able experimenter without quenching his speculative powers. He quickly learned by wincing under gibes to conform theories determinedly to facts, and he rapidly acquired adequate experimental technique. His first crude experiments were rapidly succeeded by his more enduring, and permanently famous, researches into nitrous oxide.

The future 'laughing gas', that became a furore some twenty-eight years after when eager clients hurried to inhale it, was discovered by Priestley in 1772. He obtained it first by enclosing nitric oxide, as this gas was subsequently and permanently called, over a paste of iron filings, sulphur, and water. The volume of the gas diminished, and a candle burned 'with an enlarged flame' in the resulting 'air', which was nitrous oxide, but animals died in it. Priestley subsequently procured the new 'air' from zinc and 'very dilute spirit of nitre' (dilute nitric acid). When Davy directed his medicinal search at the Pneumatic Institution to this gas in 1799, Berthollet had previously prepared it, in 1785, by heating nitrate of ammonia.

The 'pleasure-producing air', as Davy once called

it in a letter, was frequently re-christened. Priestley called it 'dephlogisticated nitrous air'; after Lavoisier it was called the 'gaseous oxyd of azote'; Davy's first name for it, 'nitrous phosoxyd', was replaced during 1799 in the records of the Medical Pneumatic Institution by 'nitrous oxide', and this was its final name. It had received a fifth name when Dr. Mitchell called it 'oxide of septon'. Dr. Mitchell diversified hopes for pneumatic cures through the introduction of a pneumatic terror by identifying this 'oxide of septon' with the 'principle of contagion'. This 'theory of contagion', that this gas was responsible for spreading disease, had already directed Davy's attention to 'nitrous phosoxyd', as he then called it, in March 1798, before he went to Bristol. He then made 'a few coarse experiments on small quantities of the gas procured from zinc and diluted nitrous (nitric) acid'. He satisfied himself that Dr. Mitchell was mistaken about his 'principle of contagion', and even breathed it, heavily diluted with common air, without any remarkable consequences. His 'duty to investigate the physiological effects of aëriform fluids' at the Medical Pneumatic Institution led him to resume the research in the March of 1799.

His new experiments were first made upon the impure nitrous oxide from zinc and 'nitrous acid'. He obtained it 'in a state of purity' in April by heating nitrate of ammonia. When Davy published his researches on the combinations of oxygen with azote

(nitrogen) in the summer of 1800, he had adequately disclosed the composition of nitrous oxide. The 'decomposition of nitrous oxide by charcoal' illustrates his analytical methods. Purified charcoal was ignited in nitrous oxide 'standing over pure mercury'. Inflammation was secured from 'the focus of a burning lens'. The quantities of nitrogen and carbonic acid produced provided the necessary data. When Davy had performed many such experiments with charcoal and other deoxidizing agents, and taken a mean value, he had secured a very adequate composition for nitrous oxide, *alias* dephlogisticated nitrous air, *alias* gaseous oxide of azote, *alias* oxide of septon, and *alias* nitrous phosoxyd. He found 36·7 grains of oxygen and 63·3 grains of nitrogen in every 100 grains of the gas. The modern figures are 36·35 and 63·65.

The gas, however, was carefully examined because it was suspected of having medicinal powers. When Davy obtained it pure in April 1799, Southey had experienced some giddiness, and a slowed pulse, by breathing it in 'a diluted state'. Davy courageously resolved 'to inspire it in its pure form'. A preliminary experiment on April 11 satisfied him that 'the gas was respirable'; on April 16, in the presence of Dr. Kinglake, he breathed three quarts of it; and the next day, when Dr. Beddoes was present, he 'breathed four quarts of nitrous oxide from and into a silk bag'. Nicholson's *Journal*, then the recipient of many scientific confidences, announced from Davy in May

that the 'theory of contagion' was false, because the gaseous oxide of azote could be quite safely respired when quite freed from nitrous gas (nitric oxide). Patients and curious inquirers were soon busy breathing nitrous oxide. 'We have seen this gas inhaled', ran a paragraph in the February issue of the *Philosophical Magazine*, 1800, and it seems to produce intoxication through the lungs, as alcohol does through the stomach. Dr. Beddoes assured readers of the March issue, in a letter to the editor, Alexander Tilloch, that the gas had 'fully maintained its character'. The worthy doctor, who had inferred its power to cure palsy, still trusted it to remove hemiplegia. Cautious experiments on a 'few hysterical females' suggested that larger doses would induce 'fits', as the doctor had previously surmised. There had been no 'untoward accident', the reassuring letter added, 'in the many hundred trials lately made with the gas'. A later letter, dated June 8, 1800, explained that the 'printing of Mr. Davy's researches on nitrous (gaseous) oxyd' would be finished that week. When Davy summarized the physiological action of nitrous oxide in his 'conclusion', the gas seemed to be a possible anaesthetic for minor surgical operations. He left the description 'of its agency in paralytic affections' to a future, and speedy, publication by Dr. Beddoes. Pneumatic chemistry would doubtless serve medicine well in the future, but this branch of the healing art was yet in its infancy. Davy had experimented too ruthlessly upon himself

to be too credulous towards pneumatic cures, or too precipitate with them. His final warning was needed, for various gases were temerariously applied, and their benefits credulously reported, during this period of pneumatic fervour.

The name 'laughing gas' commemorates the laughter and capers that were often recorded of Davy's subjects. Different individuals had different experiences. Three quarts of the gas threw one lady into a short trance, and two young ladies into a state of pleasure. One paralytic, who was questioned about his feelings after breathing nitrous oxide, replied 'I do not know how, but very queer'. Another said, with more ecstasy, 'I felt like the sound of a harp'. Many found Paradise in the 'silk bag', and many thrilled subjects refused to surrender it. Coleridge experienced 'a pleasurable sensation of warmth' during his first two trials, a few seconds of 'great extacy' at the end of the third, and 'highly pleasurable sensations' during the fourth. Many years after, in his *Table Talk* of 1830, he spoke of the 'voluptuous sensation'. No exhaustion, he then said, succeeded the excitation by nitrous oxide. On one occasion Davy himself was transported when, after due preparation, he breathed about ten quarts of the 'unmingled gas'. External things faded, 'trains of vivid visible images' swept through his mind, and he lived, for perhaps three and a half minutes, in a new world amid new discoveries. When Dr. Kinglake removed the bag, Davy's emotions were still

'enthusiastic and sublime'. As the transport faded he strove to disclose his vanishing vision in the exclamation 'Nothing exists but thoughts!—the universe is composed of impressions, ideas, pleasures, and pains!' The effects of the gas, however, were 'very different at different times' upon himself, as they also differed with different people. Nitrous oxide is still a useful anaesthetic, but it lost its vogue as a gate to Paradise. It effectively fixed the public eye upon the brilliant young experimenter who was to stand constantly before it during his career. The historical Thomson even said, though perhaps with his eye too much upon a moral, that 'this fortunate discovery perhaps contributed more to his celebrity, and to his subsequent success, than all the sterling merit of the rest of his researches—so great is the effect of display upon the greater part of mankind'. It did make him immediately famous, and took him, via Bristol, from Penzance to London, for neither the excitements of nitrous oxide nor the brilliance of Davy was to be reserved for the Pneumatic Institution. His discovery, though it was 'fortunate' in securing public interest, was also a genuine achievement: it required both chemical skill to investigate the gas and courage to inhale it.

Pneumatic inhalations nearly cost Davy his life on one or two occasions, and he required a recuperative visit to his native place before he resumed work at Bristol in the winter of 1800. The Royal Institution of London, however, soon claimed him, and he gave

his first lecture there on April 25, 1801, as 'Mr. Davy (late of Bristol)'. Nitrous oxide had not been forgotten when it added a gusto to his lecture on respiration on June 20. Those who wished could breathe it after the lecture, and some did. 'The irresistible tendency to muscular action produced by this gas' entertained the spectators. A Mr. Underwood was so transported by it that the 'breathing bag' had to be snatched forcibly from him. Nitrous oxide conspired with the stars in their courses to make Davy famous, but his own powers, and his own industry, that gave nitrous oxide its vogue, were now to seize his great opportunity.

The Royal Institution had been founded in 1799 to convey scientific knowledge and benefit to the poor; in 1807 the *Edinburgh Review* congratulated Davy on withstanding 'the enervating influence' of 'that thick medium of fashionable philosophy'. Davy himself was responsible for making it a haunt of fashion. When Rumford's doubts of the young man were effectively removed by his opening lecture, a thronged lecture-room, compliments, and public solicitude soon made Davy and the Royal Institution a vogue of London. He was rapidly raised from Director of the Laboratory and Assistant Lecturer in Chemistry, as he was first appointed, to Lecturer in Chemistry at the Royal Institution on the first of June, 1801. His Introductory Discourse to a Course of Lectures on Chemistry, on January 21, 1802, when he described the value of science to

society, completed the rising vogue of lecturer and institution. Both the institution and himself intended an ultimate professorship. With customary rapidity he was confirmed in this on May 31, 1802. His popularity steadily increased as long as he lectured in the Theatre of the Royal Institution till 1812. His audience grew with his fame until at the last, according to John Davy, 'it was scarcely less than 1,000'. Coleridge attended the lectures to multiply his metaphors, many to multiply their social ties, and some, it may be presumed, to multiply their knowledge. The Royal Institution was threatened with inanition when Davy joined it; it was threatened by a flood of fashion when he lectured.

Davy discovered as well as lectured. The *Edinburgh Review*, in 1811, had not 'made a point of following this excellent chemist through his various and important discoveries, as regularly as the nature of our publication will admit' simply because he filled his lecture-room with fashion, or even with savants. He made the laboratory of the Royal Institution as famous in the annals of scientific discovery as he made its theatre a brilliant society tryst. The Royal Institution, however, required other duties from him than the prosecution of the researches that made him permanently famous. The institution had been founded to make science useful as well as attractive. The Art of Tanning was useful, and Davy's lectures could make it attractive. He was instructed to deliver such a course, and he did

begin it on the second of November, 1801. 'Respectable members of the trade', whom Proprietors of the Institution recommended, would be admitted gratis. The young man of twenty-two, who had probably never been inside a tannery, absented himself by instruction from July to September to study practical tanning, did much experimental work, gave the lectures, and contributed a Memoir on the subject to the *Philosophical Transactions* of 1803. Davy was no laggard then, nor did social dissipation seem to stay his subsequent scientific course when his friends were fearing its detriment.

He also studied agricultural chemistry. In 1802 the Board of Agriculture secured from him a course of lectures 'On the Connection of Chemistry with Vegetable Physiology'. He continued to deliver such courses every session till 1812. His *Elements of Agricultural Chemistry*, based on these lectures, was first published in 1813. His place in the public eye probably drew the notice of this book from the *Edinburgh Review*. Its popularity, 'both in this country and the Continent', required a fourth edition in 1827.

Geology added a third inroad on his time to the inroads of Tanning and Agriculture. No systematic lecturing had preceded his first course on this subject in 1805, nor had any elementary book been written upon it. Two papers in the *Philosophical Transactions* for 1805, one on *Wavellite*, and the other on methods of mineral analysis by boracic acid, recorded a few of his 'geological enquiries'. He

made many others, and collected much dispersed material into 'a body of geological knowledge'.

Davy's election to the Fellowship of the Royal Society in 1803, when he was nearly 25, expressed his rapid rise to fame. It added another to his many activities, for the President afterwards made him secretary. Davy might be suspected of doing too many things to make any great discovery, but neither lecturing, nor tanning, nor agriculture, nor geology, nor secretarial work, nor social engagements, nor all combined prevented him either from making the famous electro-chemical discoveries that he announced in his first Bakerian Lecture to the Royal Society on November 20, 1806, or from isolating the alkali metals in 1807. He surveyed the scientific concepts of his day in his lectures, and he developed those concepts in his laboratory.

III

ELECTRO-CHEMICAL SUCCESSES

'GALVANIC phenomena' were already significant enough in 1801 for Davy to give a course of lectures on this 'new branch of philosophy' at the 'Royal Institution of Great Britain' in London. This 'new branch' was possible through the 'pile of Volta'; it was in being because Nicholson and Carlisle had decomposed water with the pile. In 1801 Davy told an audience that the experiment of Nicholson and

Carlisle had disconcerted chemists: the decomposition of water, and of other substances, if it were a fact, was an utter enigma. All analogies failed at the moment, said the young lecturer, and 'perfectly new views of corpuscular action' would probably be needed. In 1826, twenty-five years after, *Sir* Humphry Davy, as he then was, told another audience that 'the true origin of all that has been done in electro-chemical science was the accidental discovery of MM. Nicholson and Carlisle, of the decomposition of water by the pile of Volta, April 30, 1800'. The decomposition of water by the pile was less a paradox then, though there were still problems to solve, and Davy himself, the chief performer with the pile, had done very much of what had been done.

The Italian physicist Volta announced his 'memorable discovery of the pile' to English scientists in a letter dated March 20, 1800. On June 26 his account was read to the Royal Society. Each pair of the Voltaic Pile consisted of a disk of zinc or tin laid on another of silver or copper. A card, soaked in brine, separated each pair. The pairs and their cards were arranged in a pile so that the regular alternation zinc-copper, card, zinc-copper, card, zinc-copper occurred. Increasing the number of pairs, by increasing the number of zinc-coppers and cards between the top and bottom pairs, increased the power of the pile. If metal strips from the terminal metals, one from the top zinc, and one from the bottom copper in the example, were dipped into separate

bowls of water, a shock was experienced by dipping the hands in the bowls. The moistened hands could also be placed, one upon each, on the two ends of the pile.

Volta also replaced the paired disks by zinc-copper strips, for example, and the moistened cards by vessels of wood, earthenware, or glass, that contained brine or dilute alkali. Each vessel of the battery then contained a zinc and copper strip separated by a layer of solution, the zinc strip of each vessel, or cell, being soldered to the copper strip of the *adjacent* vessel. The immersed parts of the metals, Volta advised, should be plates 'of an inch square' to present surface to the liquid. The 'rest of the arc' might be 'a simple metallic wire', and might also be 'a third metal different from the two immersed in the tumblers'. Volta called this 'new apparatus' a *couronne de tasses*. The final strip at one end of the row of cells was copper, and zinc at the other end, just as in the pile. This was the first form of the now familiar 'battery'. The 'columnar apparatus', because it was 'active of itself' without any excitation, and acted incessantly, like the 'natural electric organ of the torpedo or electric eel', he called the *artificial electric organ*. Did not the continued shocks, renewed by each touch, received from the pile, produce the same torpor in the limbs as the torpedo produced!

Meno, in Plato's dialogue, compared Socrates to the 'flat torpedo fish, who torpifies those who come near him with a touch'. John Davy remarked that

'the powers of the torpedo and gymnotus to give shocks had been long known'. In 1774 Walsh discovered 'that the effect was electrical'. Since the dissecting knife of John Hunter had exposed the arrangement of the 'electrical organs' of these animals in columns, Volta very naturally compared his 'columnar apparatus', or pile, to the 'natural electric organ of the torpedo or electric eel', and called it the *artificial electric organ*.

Nicholson and Carlisle made prompt use of the new instrument. They had decomposed water by the pile before Volta's communication had been read to the Royal Society in June. They used 'a pile of thirty-six half crowns with the correspondent pieces of zinc and pasteboard', and also piles with more 'sets'. In the culminating experiment two platina wires were inserted, through a cork at either end, in a tube containing water. The two points of the two wires were *separated* in the water by an inch or so. When the platina wires were connected with the two ends of a pile, gases streamed from them. The experiment was extended by collecting the gases from either wire separately. They were collected by placing the platina wires 'under separate phials full of water inverted in a saucer of water'. The decomposition was slow: in thirteen hours the whole volume of collected gas was 1·17 cubic inches. The gas from the negatively charged 'silver side', from the wire connected with a half-crown, had approximately double the volume of the gas collected from

the positively charged 'zinc side'. This corresponded to the presumed 'proportions in bulk' of 'the constituent parts of water'. Examination indicated that oxygen came off from *one* wire, and hydrogen from the other.

Chemists stared in amazement at oxygen and hydrogen bubbling off from *separated* wires. A letter from 'a correspondent' to Tilloch, that was published in the February issue of the *Philosophical Magazine* in 1801, expressed a general incredulity. Two glasses of water, connected by a siphon, could be substituted for the single saucer of the original experiment. If the platina wires, connected with the two sides of the pile, were dipped in the two separated glasses, one in each, the oxygen and hydrogen still fizzed off separately—one in each glass. 'If a particle of water is composed of a particle of oxygen and a particle of hydrogen', exclaimed the 'correspondent', there will be some 'rapid currents'. When oxygen streamed off at one wire, the hydrogen with which it had been combined had apparently to scamper through the siphon to stream off at the other wire. If the water was decomposed at one wire, and the hydrogen came off there, the oxygen previously combined with it had apparently its own scamper through the siphon to fizz off at its own wire. These scampers through the siphon appeared to be impossible, and it seemed to the 'correspondent' that the water was not actually decomposed. The oxygen bubbled off the *positively charged*

platina wire. Positive electricity had therefore combined with water to form oxygen gas. 'Hydrogen air', analogously, was water charged, or combined, with negative electricity. When the two mixed gases were exploded by a spark or a light, the positive electricity of the oxygen united with the negative electricity of the hydrogen to form fire, and the waters of both gases, stripped of their electricities, were precipitated as water. The correspondent presumed the reaction

$$\text{positive electricity} + \text{water} = \overset{\text{OXYGEN}}{(\text{positive electricity} + \text{water})}$$

to occur at the positive wire, and the reaction

$$\text{negative electricity} + \text{water} = \overset{\text{HYDROGEN}}{(\text{negative electricity} + \text{water})}$$

to occur at the negative wire. When the two gases combined the reaction was, using the signs + and − for positive and negative electricities respectively,

$$\overset{\text{OXYGEN}}{(\text{water}+)} + \overset{\text{HYDROGEN}}{(\text{water}-)} = \overset{\text{FIRE}}{\text{water} + \text{water} + (+\,-)}.$$

This explanation was not uncongenial to the mind of the time: 'Ritter, and some ingenious English inquirers', Davy noted, adopted it. Even in 1808 Davy himself was lingering over a similar explanation, though he never adopted it definitely. He inverted the charges on the two gases in his suggestion. He supposed the oxygen to be attracted to the *positive* wire because it was *negatively* charged, and the hydrogen, because it was *positively* charged, to move

to the *negative* wire. Ritter's exposition had a vogue for the moment, when Nicholson and Carlisle so disconcerted chemists, because it avoided the enigmatical scampers of oxygen and hydrogen to be in time at their own wires.

Davy had not left Bristol when the experiment of Nicholson and Carlisle astonished the scientific world. The mysterious appearance of hydrogen and oxygen at separated points diverted his experiments at the Pneumatic Institution. A pile constructed for Dr. Beddoes was at his disposal. He satisfied himself 'that oxygen and hydrogen, nearly in the proportions required to form water, could be separately produced' from separate 'quantities of water', if the waters in the separated vessels were suitably connected. Davy played the part of the 'syphon' mentioned by 'a correspondent' by placing his fingers in the glasses. This connexion was sufficient, as was connexion by 'dry metallic conductors and muscular fibre'. Davy could neither experiment nor think himself out of the primary perplexity at the moment. He was still doubtful in the December of 1800 whether water were genuinely 'decomposed in galvanic processes'. If it were, one at least of its constituents could pass, rapidly and invisibly, through metallic substances, water, or many organic bodies. Davy had connected a platina wire with one end of a pile, and placed it in a glass of water. He had connected another platina wire with the other end of the pile, and placed it in water in a second glass. When

he connected the two glasses with his fingers, or a wire, or a muscular fibre, oxygen fizzed off from one platina wire, and hydrogen from the other. If the two gases had been liberated from a portion of water one at least of them must have passed through himself, or through the connecting metallic wire or fibre, as well as through a stretch of water, to arrive at its own wire. So, at least, it seemed, and so incredible the assumption of decomposed water seemed to be. Davy was still confessing this moment of frustration when he lectured on 'galvanic phenomena' after his transfer to the Royal Institution in 1801. Nor were the hurrying habits of oxygen or hydrogen the only enigmas of the pile.

When water was 'galvanized' not only did oxygen and hydrogen appear disconcertingly separated, but acid matter presented itself at the positive wire, and alkaline matter at the negative. These acids and alkalies at the 'electrified surfaces' promoted suspicions and theories. Cruickshank suspected 'nitrous' (nitric) acid and ammonia; Brugnatelli presumed an 'electric acid'; some experiments suggested that muriatic acid (hydrochloric acid) or even oxymuriatic acid (chlorine) was produced. These, and other acids or alkalies, were supposed, like the oxygen and hydrogen, to be electrified waters. In 1805 suspicions were still rife in both Italy and England that muriatic acid or alkali could be generated from water by galvanic action. Davy did not dogmatically deny such versions, but his researches had been

steadily clarifying the enigmas of the pile. Neither Davy's multifarious duties, nor his social popularity, had prevented him from making significant electro-chemical discoveries when he delivered his first Bakerian Lecture to the Royal Society on November 20, 1806. It was 'On some Chemical Agencies of Electricity'. In his *History of Chemistry*, 1830–1, Dr. Thomas Thomson considered 'this paper not merely as the best of all his own productions, but as the finest and completest specimen of inductive reasoning which appeared during the age in which he lived'. Berzelius included it among the greatest memoirs that had ever enriched the theory of chemistry. It received a striking honour almost immediately. Napoleon Bonaparte, when First Consul, had founded a medal 'for the best experiment' on 'the galvanic fluid' in each year. Though France and England were at war, the Institute of France awarded the medal to Davy twelve months after he published his Bakerian Lecture.

In his 1806 paper Davy had decided, by successive eliminatory steps, that 'chemically pure' water decomposed only into oxygen and hydrogen. Any other apparent products were derived either from extraneous matter in the water, or from the material of the apparatus. In the fundamental experiment two platina wires, connected with either end of the pile, were plunged into separate vessels of water. If the vessels were connected by a conductor, oxygen appeared at the positive wire, and hydrogen at the

negative. Dr. Wollaston suggested 'well-washed asbestus' to connect the 'tubes together'. The immediate problem was the simultaneous appearance of acid matter with the oxygen at the positive wire, and the simultaneous appearance of alkaline matter with the hydrogen at the negative. Distilled water and gold vessels, or 'tubes', reduced the acid and alkaline products to a bare minimum. Some 'nitrous acid' did appear in proportion to the length of the operation, and also a limited quantity of 'volatile alkali' (ammonia). When the water was deprived of air, and kept in contact with hydrogen, 'neither of the portions of water altered in the slightest degree the tint of litmus': no acid nor alkali appeared if contributory extraneous matter were rigidly excluded from the water, and the gold tubes were used to exclude contributory extraneous matter from the apparatus. Glass tubes furnished soda, even 'tubes of agate' were sources of 'saline matter' until they had been 'often used', but gold tubes contributed neither acid nor alkali. Davy had resolved one problem, for the appearances of 'acid matter' round the 'positively electrified metallic surface' or of 'alkaline matter' round the negative wire, had been referred to the decompositions or reactions of substances other than water. Such decompositions or reactions now 'offered some serious objects of inquiry'.

Lavoisier imposed upon chemistry a very insistent version of the constitution of *salts*. Every acid, on this version, contained oxygen. Much experiment

was needed, and much reluctance required dispersal, before chemists realized that some acids did not contain any. Sulphuric acid, for example, was regarded as a compound of oxygen and sulphur. It corresponded to the sulphur trioxide of present chemistry. Other acids were analogous compounds of oxygen with other substances. The *acids* of Davy's day corresponded to the modern *anhydrides*: substances that contain oxygen, and give acidic substances when they are associated with water.

Lavoisier meant by an *oxyd* a substance that contained oxygen, but was not oxygenated, or sufficiently oxygenated, into an acid. An acid and an oxyd combined into a *salt*. Blue vitriol, for example, copper sulphate, was

$$\underbrace{\underbrace{(\text{copper}+\text{oxygen})}_{\text{OXYD}}+\underbrace{(\text{sulphur}+\text{oxygen})}_{\text{ACID}}}_{\text{SALT}}$$

All salts were constituted on this general plan, though the metal, or substance, corresponding to the copper in other salts might not be known, nor might the substance corresponding to the sulphur be known either. When experiments with the pile began, the oxyd part of many salts was known, or considered, to be alkaline.

Nicholson and Carlisle very quickly observed that when 'certain metallic solutions' were submitted to the action of the pile, alkali appeared near the negative wire. Cruickshank, and others, also soon found

that salts in solution dissevered into acids and alkalies. Parallel inquiries on the Continent and in England steadily established that when salts in solution were subjected to the action of the pile acids appeared with the oxygen at the positive wire, and alkalies appeared with the hydrogen at the negative. These parallel inquiries proceeded very independently at first, and, in any event, the researches of Davy are the present theme. Davy had proved, in 1806, that apparent transformations of water into acids and alkalies by the pile resulted from dissolved saline, or gaseous, matter. The decomposition of a salt into an acid and an alkali was as intelligible as the decomposition of water into oxygen and hydrogen, because it contained acid and alkaline constituents, just as water contained the gases. 'Sulphate of lime', for example, a salt used by Davy in his experiments, was composed of lime and sulphuric acid. Its constitution was presumably

$$\underbrace{\overbrace{(X+\text{oxygen})}^{\text{LIME}}+\overbrace{(\text{sulphur}+\text{oxygen})}^{\text{SULPHURIC ACID}}}_{\text{SULPHATE OF LIME}}$$

where X was an unknown metal. The separation of this compound into its constituent lime and constituent sulphuric acid by the pile, if there was evidence for it, was no enigma. The collection of the lime at the negative wire, and of the acid at the positive, repeated the perplexing separation of the oxygen and hydrogen from water. Lime and sulphuric acid, or

other alkalies and acids, seemed to scamper through siphons or connecting conductors or stretches of water just like hydrogen or oxygen. Davy systematically proved that a salt was decomposed into an acid and another substance that was very often an obvious alkali. He also systematically proved that the acids and alkalies *seemed* to travel through liquids and solids in a paradoxical way. These inquiries constituted the next experimental step in his 1806 paper.

Davy connected 'two cups made of compact sulphate of lime' by fibrous sulphate of lime. The connecting fibre was moistened with 'pure water'. The cups were filled with water, and 'platina wires from the Voltaic battery of 100 pairs of plates of six inches' plunged separately into them. The sulphate of lime was soluble enough in water to form an effective solution in each cup. A 'saturated solution of *lime*' collected round the negative wire in one cup, and a 'crust of lime' partially covered it. 'A moderately strong solution of sulphuric acid' simultaneously surrounded the positive wire in the other cup. In this, as in many analogous decompositions effected by Davy, the 'acid matter collected' round the 'positively electrified surface', and the alkaline round the negative. The galvanic current appeared to be able to decompose 'a weak solution of sulphate of potash', for example, *completely* into its constituent alkali and acid. Metals, or metallic oxides, could behave like alkalies.

The 'electrical transfer' of the alkali to the negative wire, and of the acid to the positive, seemed to be

very effective. Davy varied his cups and contents in various ways. The sulphate of lime cups had been convenient for supplying solutions of the salt, but in other experiments the cups were composed of resistant material, such as gold, that would not affect the results. He placed, for example, distilled water in one such 'tube', and a solution of sulphate of potash in the other. When the negative wire from the pile was immersed in the solution, and the positive wire in the distilled water, sulphuric acid appeared in the latter place. The acid had apparently crossed by the connecting fibre, frequently of Wollaston's 'well-washed asbestos', from the solution to the water. It was compelled to pass through an intervening vessel that contained distilled water tinged with litmus. It did not redden it, nor did alkali, when it was analogously transferred through turmeric, change the tint. The litmus reddened, or the turmeric browned, near the wire where the acid or alkali was collecting. The appearing colour only diffused through the vessel where the acid or alkali was collecting round the wire—it never reached the interposed vessel. The electrically transferred acid also crossed through interposed alkaline solutions, and alkalies similarly crossed through acids.

Hydrogen, alkaline substances, metals, and some metallic oxides were apparently attracted by negatively electrified surfaces, and repelled by positive. Oxygen and acids were oppositely attracted or repelled. Since acid was found in alkaline solutions

through which it had passed on its way to the positive wire, 'the repellent and attractive energies' were probably 'communicated *from one particle to another particle* of the same kind': locomotion appeared to be the consequence of a *conducting chain*. There might be a succession of 'decompositions and recompositions throughout the fluid' because *insoluble* compounds were precipitated when barytes was passed through sulphuric acid, or muriatic acid (hydrochloric acid) through sulphate of silver solution. Davy had thought his way through the enigmatical travels of substances under the power of the pile without resorting to the somewhat desperate conclusions of Ritter and others. Grotthus, in 1805, suggested the chain of decompositions and recompositions that brought substances to their appropriate wires. His hypothesis is familiar to readers of textbooks on physical science. Davy had apparently thought very similarly along his own line of discovery. Some water near the positive wire, for example, lost its oxygen to the attracting surface. That left some free hydrogen there. Some water near the negative wire simultaneously lost its hydrogen to it, and that left some free oxygen there. Shuffles of oxygen and hydrogen through the liquid arranged new partners for the freed oxygen at the negative wire, and for the freed hydrogen at the positive wire, and so the process continued. Exposition was not yet complete, nor was Davy wholly satisfied, but the separation of galvanic products at different wires

was, at any rate, deprived of part of its mystery without resorting to Ritteran notions. Davy was to glance again at Ritteran notions during speculative struggles, but his famous research had definitely advanced electro-chemical science and prepared the way for its future developments.

Davy concluded his paper by discussing the relations of 'Electrical Energies' to 'Chemical Affinities'. The two had very obviously suggested that they were very intimately connected. If 'chemical affinities' united two gases into water, and 'electrical energies' separated them again, it was tempting to presume their intimate connexions. Davy emphasized the clues to this connexion that his own researches had provided. Chemistry worked these clues hard, sometimes no doubt too hard, but now, a hundred years after Davy's death, the presumed connexion between chemical affinities and electricity is perhaps even more intimate than Davy dreamed.

Davy announced the dramatic sequel of his first discoveries to the Royal Society in his second Bakerian Lecture on November 19, 1807. The analogies of the day suggested that the decomposition of sulphate of potash, for example, by the galvanic current from the pile, was represented by

$$\underbrace{\underbrace{\text{POTASH}}_{(\text{base}+\text{oxygen})}+\underbrace{\text{ACID}}_{(\text{sulphur}+\text{oxygen})}}_{\text{SULPHATE OF POTASH}} = \underbrace{\text{POTASH}}_{(\text{base}+\text{oxygen})}+\underbrace{\text{ACID}}_{(\text{sulphur}+\text{oxygen})}.$$

Davy attempted to advance the decomposition one stage further by decomposing the separated potash into the unknown 'base' and oxygen. Since he had potash at hand (the present potassium hydroxide, or caustic potash), he submitted its concentrated solution to the 'highest electrical power' he 'could command'. When 'a combination of Voltaic batteries belonging to the Royal Institution' only decomposed the water into oxygen and hydrogen, he used 'potash in igneous fusion'. He placed 'a small piece of pure potash', previously exposed to the atmosphere for a few seconds to make it moist enough to conduct, 'on an insulated disk of platina'. The 'negative side of the battery' was connected with the disk, and a platina wire from the positive side made contact with the 'upper surface of the alkali'. Electricity was 'the common agent for fusion and decomposition'. On the sixth of October, 1907, he finally liberated 'small globules' of the 'basis of potash' that shone and rolled like quicksilver. Some of them immediately burnt with explosion and flame, others tarnished more slowly till a white film covered them. Davy 'bounded about the room in extatic delight' when 'the minute globules of potassium burst through the crust of potash' and then flamed. A few days later, soda, that was more refractory, submitted similarly to the power of the pile.

Newly isolated substances require names. When the suspected 'bases of potash and soda' were disclosed by the 'Voltaic battery' two christenings were

needed. The *Edinburgh Review* had heard whispers of 'potagen' and 'sodagen'. Davy called the two newly discovered substances 'potassium' and 'sodium'. These names committed science to no theories, for they simply denoted the metals produced from potash and soda. Davy had again combined genuine scientific discovery with dramatic disclosure. Two new metals that decomposed water simply by touching it made as dramatic an appeal as the powers of nitrous oxide. Davy constantly added to the captivation of his eloquence an impressive appeal through his discoveries.

A misfortune helped the drama of discovery to increase his fame and popularity. The Rev. Dr. Dibdin announced in his Introductory Lecture on January 18, 1808, at the opening of the Royal Institution, that Mr. Davy had been for five weeks between life and death. This severe illness, that did not leave him convalescent for nine weeks, interrupted Davy's researches between the decompositions of the fixed alkalies, potash and soda, and the decompositions of the alkaline earths. John Davy described it as 'the golden period of his life', for it made him the still more famous and popular centre of anxiety and inquiry. When Davy returned to his laboratory in 1808, the hero of a laboratory drama and the centre of public solicitude, he soon required four more names for four newly isolated metals.

Davy had decomposed potash by compelling the potassium to a negatively charged surface of platina.

The naked metal was sensitive to the oxygen and moisture of air. By mixing red mercuric oxide with the potash mercury could be compelled to the negative surface simultaneously with the potassium. Thus a mixture of potash and oxide of mercury, 'electrified in the Voltaic circuit', procured an *amalgam* of potassium. This amalgamation considerably protected the potassium from the action of air. This device of presenting mercury to the generated metal was applied to the decompositions of 'barytes, strontites, and lime'. Davy hoped to obtain a metal from lime as he had obtained one from potash, and two others from the other earths, or presumed oxyds. Acting on a letter from Berzelius, he negatively electrified mercury by connecting it with the negative side of the Voltaic battery during the galvanic decomposition, in contact with barytes, or lime, with distinct results. Strontites also gave a rapid result, but when he tried to drive a metal out of magnesia into amalgamation with the mercury the coercion was less effective. The amalgams, however, by decomposing water, and oxidizing into earths, indicated the isolations of the four metals. Each earth had obviously lost a metal to the mercury to amalgamate with it. This amalgamation protected the generated metals sufficiently from air and moisture for their presence to be detected. The decomposition of water by the amalgams showed that the metals were present, for mercury alone did not do it. As the metals oxidized back into lime or other 'earth'

there were visible signs in the amalgams of the oxidations. The metal from lime was called 'calcium', and the names 'barium' and 'strontium' were given to the metals from 'barytes' and 'strontites' respectively. The name 'magnium' for the metal derived from magnesia was afterwards altered to 'magnesium'.

Davy reported his fresh results to the Royal Society on June 30, 1808; the two *Reviews*, the *Edinburgh* and the *Quarterly*, duly reported his discoveries to a wider public. As *Hamlet* is often said to act itself, so a comment by the *Edinburgh Review* suggested that, in a sense, the decomposition of water by the 'pile of Volta' acted out its own inevitable results. Both *Reviews* complimented Davy by comparing him with Sir Isaac Newton. Davy's Bakerian Lectures, according to the *Quarterly* in 1812, were as superior to Newton's *Optics* 'for a series of well-designed experiments and brilliant discoveries' as the *Principia* was superior to any other human work for the application of theory to results. The *Edinburgh*, in 1808 when Davy had isolated the alkali and alkaline earth metals, considered that he had made more discoveries than any man since Newton. Yet nothing in his record deserved the name of genius, the literary pontiff added, for he owed more to the powerful instrument placed in his hands than did Newton, who created his own tools. This was equivalent to saying that Davy was rapid and brilliant—'brilliancy' and 'rapidity' were *Edin-*

burgh Review epithets for Davy—but that the Voltaic battery helped him to be rapid, and the decomposition of water showed him how to be brilliant. Water cleaving into its constituent gases under the power of the pile obviously did imply that other substances could be similarly decomposed. The decomposition of water cried for a pile, a powerful pile if need be, to be applied to other substances. Nicholson and Carlisle did give the cue, the Royal Institution did provide the instruments, but Davy supplied the rapid brilliancy. He did, no doubt, owe much to 'the powerful instrument in his hands', for it was powerful enough to effect his purpose. The 'powerful instrument' also owed much to him, for he studied out its powers, constructed it appropriately, and applied it discerningly. The *Edinburgh Review* itself, referring in 1807 to Davy's first Bakerian Lecture, wrote: 'The chain of experiments by which he removes all the difficulties and obscurity that hitherto hung over the changes produced in water by galvanic action, is surpassed by no inquiry of the kind in modern times, for closeness, copiousness, and minute accuracy.' Davy was certainly a great discoverer, for all agreed on that; he certainly achieved fame, for that was guaranteed by discussions over it; it is perhaps idle to debate whether the pile owed more to him, or he to the pile.

IV

A PERIOD OF PERPLEXITIES

WHEN Davy discovered the new metals, and disclosed their striking properties, he acquired a pet idea. He announced it to the Royal Society on June 30, 1808, when he had described his isolation of metals from the alkaline earths. The new metals suggested to him a 'theory for the phenomena of volcanoes, the formation of lavas, and the excitement and effects of subterranean heat'.

Volcanoes were one theme of mythology, but in more rational versions a central fire was supposed to burst through the apertures of flaming mountains. When little is known, or interpretation is difficult, men catch eagerly at apparently possible explanations. Nicolas Lémery, a French chemist who lived from 1645 to 1715, had made 'equal quantities of the filings of steel and sulphur powdered' into a 'Paste with water'. If the paste was left to 'ferment' it flamed. When the earth trembled, and fires burst forth, as at Vesuvius or Etna, 'sulphurs' might have penetrated iron in mines to take flame as 'in the present operation'. It seemed to Lemery that 'the fermentation which happens at the meeting of Iron and Brimstone may be able to raise the earth in some places and to burst it asunder'. The 'combustion of mineral coal' was 'one of the most current' theories when Davy proposed another version of earthquakes

and volcanoes. His theory was modelled on an impressive experiment of his own, as Lemery's was formerly modelled on the experiment with iron filings and sulphur. He announced it in 1808, pronounced it at intervals, and renounced it with reluctance shortly before his death in 1829.

The new metals reacted violently with water or air. If 'metals of the earths' below the surface of the globe were acted upon by water or air, there might be a 'subterranean fire' to burst out in volcanoes. When Davy also surmised that electrical actions might generate these metals, or the alkali metals, below the earth's crust, he was transferring his own experiments, on an enlarged scale, to the operations of nature. Potassium, or one of the other active metals, might be electrically generated in terrestrial depths as it was in his laboratory by the pile. It might also flame out through a volcano under the action of water and air, as it flamed in his experiments. The earth might well heave under such violent reactions, and lavas might result in the furnace of their violence.

Davy petted this idea, though he suspected that it would not command assent, but he surrendered it at last. He was hesitating when his penultimate paper was read to the Royal Society on March 20, 1828, *On the Phenomena of Volcanoes*. The 'simple and brilliant hypothesis', as his admiring brother called it, had been in his thoughts for twenty years. There still seemed to be 'no other adequate source' for 'volcanic

fires' than 'the oxidation of the metals which form the bases of the earths and alkalies' by their exposure to air and sea-water. If the 'interior of the globe' was very hot, however, a nucleus of hot fluid matter solved the problem of volcanic fires more simply than supposititious alkali or earth metals fizzing with water or burning in air. The *Stranger*, in the third dialogue of *Consolations in Travel*, written by Davy shortly before his death, made the final reluctant surrender for Sir Humphry. It was not unreservedly surrendered, for '*some* results of volcanic fire' *might* proceed from 'the action of air and water upon the combustible bases of the earths and alkalies'. Many, and often dangerous, experiments had finally failed to confirm the long-alluring notion that they *all* could be. A 'nucleus of fluid ignited matter' was a 'general and simple cause' of volcanic action, and 'more agreeable to the analogies of things' than his own favourite notion. The hypothesis of a 'permanent central fire', as John Davy commented, was Sir Humphry Davy's final, if hesitating and reluctant, choice.

Sir Humphry Davy could relinquish an opinion, even a pet opinion, if it was supported neither by experiments nor by observation. He cherished his theory of volcanoes for about twenty years; then he deferred to data, and discarded it. Dr. Charles Daubeny accused him, after his death, of a later distaste for his pet theory because it had been adopted by a 'humble class of inquirers'. It was not, said

Daubeny nastily, 'inconsistent with what we know of his character to suppose' this. The admiring brother did not find this 'criticism reasonable', nor perhaps will those who now survey Davy's frequent plasticity of opinion find it reasonable either.

The new metals, in conjunction with other items, also disturbed chemical ideas, and, during a period of theoretical perplexities for Davy, even tempted chemists, including Davy himself, to adopt revised phlogistian theories.

The composition of potassium sulphate, for example, as

POTASH SULPHURIC ACID
(potassium+oxygen)+(sulphur+oxygen)

seemed to conform to some experimental data. The galvanic current severed the dissolved salt into potash that went to the negative wire, and into sulphuric acid that went to the positive. A powerful battery procured potassium from the fused potash itself by decomposing it further. It seemed to Davy himself, at the moment of successful discovery, 'that potash and soda are *metallic oxides*'. He had not thought thus of potash before he decomposed it. Potash was strongly *alkaline*. So was the volatile alkali (ammonia). The volatile alkali contained hydrogen and nitrogen. There might, therefore, be hydrogen in potash, and perhaps phosphorus or sulphur to correspond to the nitrogen. Davy's immediate experiments disturbed this analogy between ammonia and potash, but neither potassium nor the other metals discovered

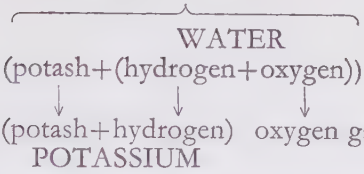
along with it were to be called elements without challenge.

According to the phlogiston theory every metal was X + phlogiston. The X might be the calx itself, or it might be an unknown base or basis. Revised phlogiston theories identified the phlogiston with *hydrogen*. Such revised phlogiston theories received an impetus when Davy had isolated potassium and the other five metals, sodium, barium, strontium, calcium, and magnesium. The new metals, especially potassium and sodium, were very *light*. So was hydrogen. Other data also favoured hydrogen in these metals. It seemed to Dalton in 1810 that the metal potassium was probably potash + hydrogen, or 'Hydruret of Potash'. The potash in the metal also seemed to him to be most probably *elementary* potash. Ordinary caustic potash, which, as Berthollet had shown, seemed to contain a definite proportion of combined water even when fused, was presumably a *hydrate* of potash. When potassium acted on water it lost its hydrogen, that escaped as gas, and potash remained in the alkaline liquid, according to the representation:

POTASSIUM
(potash+hydrogen)+water

CAUSTIC POTASH
= hydrogen+(potash+water).

When Davy galvanized caustic potash, the galvanic current decomposed it according to the scheme:



This was obviously a revised phlogiston theory, as the steps X+phlogiston, X+hydrogen, potash+hydrogen show.

The apparent disclosure of *ammonium*, that appeared to be *metallic* because it seemed to form an amalgam with mercury, and also to contain *hydrogen*, increased the suspicion that *all* metals contained hydrogen. Berzelius and Pontin had placed a solution of ammonia in contact with mercury. When the quicksilver was 'negatively electrified in the Voltaic circuit' it distended to four or five times its original volume, and became a soft solid. This seemed to be an *amalgam*, a compound of a metal with mercury, and it seemed to contain nitrogen and hydrogen derived from the ammonia. They communicated their results to Davy, who reported his research on the 'amalgam' to the Royal Society on June 20, 1808, after he had reported his decompositions of the alkaline earths. He repeated the original experiments, and also prepared the 'amalgam' by other methods. Moistened muriate of ammonia (ammonium chloride) was placed on a plate of platina, which received the positive wire from 'a large battery'. A globule of mercury, placed in a cavity of the muriate, received

the negative wire. The globule rapidly enlarged as before, shot out 'metallic crystallizations', and looked like an 'amalgam of zinc'. There was no zinc, but some other *metallic* substance was presumably present. This substance was presumably the ammonia, or derived from it, and contained nitrogen and *hydrogen*. Davy preferred to depend his examination upon the 'amalgam' 'procured by electrical powers', whether by this or modified methods, rather than on the more swollen samples prepared by acting directly on 'moistened muriate of ammonia' with amalgams of potassium, sodium, barium, or calcium. The unstable buttery product, that became more firmly crystalline on freezing, was difficult to analyse because it was so fugitive, but it seemed to be a genuine 'amalgam from ammonia'. Davy called the substance that formed 'with mercury so perfect an amalgam' by the name *ammonium* because its amalgamating habit implied *metallic* nature. If ammonium was a *metal*, and contained *hydrogen*, as seemed to be true, *all* other metals *might* contain hydrogen too. The metals were now distinctly threatened with a return from their status as elements, to which Lavoisier had raised them, to their original status as compounds, in which phlogistians and their predecessors had always placed them.

Davy had confided the possibility of a 'phlogistic chemical theory' to his note-book when illness kept him from the laboratory during the end of 1807 and the beginning of 1808. He had not kept it private,

for he had paused over it previously when he was discussing potassium and sodium before the Royal Society in 1807. He had distilled the 'basis of potash' (potassium) and obtained *hydrogen*. He also discovered, however, that this hydrogen might not actually come from the potassium itself. If water were present, for instance, even in small amounts, the potassium would decompose it and evolve hydrogen. This circumstance, however, did not dispel Davy's suspicions that potassium might contain hydrogen, even if experiment did not make it immediately obvious. Even at the end of 1809 he was still lingering hesitantly over the possibility that hydrogen might be 'the principle which gives inflammability' and 'the cause of metallization'. Phlogistians had supposed that inflammable bodies contained phlogiston, and that metals contained it also; Davy was now lingering doubtfully over a substitution of hydrogen for phlogiston. He never actually reverted to this opinion, as Dr. John Davy rightly insisted, but he did survey it as a possibility. This inclusion of the phlogistian alternative among various theories surveyed by Davy at this period is an index both to the puzzles of the times and to Davy's power to look in all directions for possible explanations.

When gold was heated it did not form a 'calx'. Chemists had known this for centuries. It held its phlogiston too tightly according to phlogistians; it had too little affinity for oxygen according to Lavoisier and his followers. Platinum, or 'platina'

as it was often called then, resembled gold in refusing to calx, and was subject to the same two explanations. Revised phlogistianism supposed that platina, like gold, held its hydrogen very tightly. In this it contrasted with *ammonium*, that lost its hydrogen comparatively easily. The more easily decomposed ammonium, that seemed to be metallic and to contain hydrogen, hinted that the metal platina contained hydrogen too, though it held it much more firmly. Davy suggested that hydrogen might be 'combined with the greatest energy' in platina, and with the least in ammonium, of all the metals. In most metals the hydrogen could be prompted by heat, or by other conditions, to unite with oxygen to form water. The union of this water with the residual basis of the metal produced the calx or oxide. The process was

$$\begin{array}{c} \text{METAL} \\ (X+\text{hydrogen})+\text{oxygen} \\ \downarrow \qquad \downarrow \text{ WATER } \downarrow \\ \underbrace{(X+(\text{hydrogen}+\text{oxygen}))}_{\text{CALX or OXIDE}} \end{array}$$

Davy applied similar explanations to combustible bodies that were not metals. This was phlogistianism with hydrogen instead of phlogiston. Davy did not adopt it, but he did contemplate its possibility, and some of his contemporaries adopted it, or some modification of it, for at least some metals.

Davy observed the evolution of sulphuretted hydrogen when he decomposed sulphur 'by Voltaic

electricity'. There must have been hydrogen in the sulphur because too much sulphuretted hydrogen was evolved for it to proceed from a mere impurity. He similarly suspected hydrogen in phosphorus because it evolved phosphuretted hydrogen under the power of the pile. Charcoal also seemed to contain 'a minute quantity of hydrogen in combination'. Davy discarded these results and inferences subsequently, but for the moment he suspected hydrogen in sulphur and phosphorus, as well as in charcoal.

Sulphur might be X + hydrogen. During its combustion the hydrogen would combine with oxygen to form water. The final acid product might then be X + water. Phosphorus and other combustibles might also be X + hydrogen, each substance having its own X. Similar explanations also might apply to their combustions. The X in a metal represented a 'metallic basis'. This might be true also of sulphur, and of other combustibles that did not appear to be metallic. They all, metals and combustibles, *might* each contain a private X combined with hydrogen, an X that was elementary in each case, and also metallic.

Indurated notions often resume a power after a period of apparent eclipse. The revised phlogistianism of this time may represent such a resumption, for the phlogiston theory had been very universal before Lavoisier. Perhaps it had never been completely expelled, and its versions still lingered to be roused into activity by suggestive discoveries. The

lightness of the new metals hinted at hydrogen in them. Ammonium reinforced the hint by disclosing hydrogen in an apparently metallic, or potentially metallic, body. It was possible to explain much by assuming that metals and combustibles did contain hydrogen. Davy indeed admitted that it might be as possible to explain all by this as effectively as by other suppositions. It was possible to class combustible bodies with metals by assuming hydrogen in them all, and also by assuming, though perhaps with less cogency, a 'metallic basis' in each. Sulphur, phosphorus, and charcoal did actually appear to contain hydrogen. Electro-chemical data also seemed to Davy to suggest hydrogen in inflammables. The substances presumed to contain hydrogen were electropositive: they were attracted to the 'negative surface' during decompositions by the pile or battery. Oxygen was the only presumedly elementary body that was attracted by the 'positive surface', and all other bodies, whose nature was known, that were attracted to the positive surface, contained oxygen. It seemed reasonable to suppose, therefore, that all bodies other than hydrogen that went to the negative surface correspondingly contained hydrogen. Hydrogen and oxygen were opposites; bodies containing oxygen went like it to the positive surface, those bodies that went like hydrogen to the negative surface might well contain it.

The period from 1808, when Davy returned to work after his illness, to 1810, when he disclosed the

presumptively elementary nature of chlorine, was a period of perplexities. Davy himself remarked that the use of 'novel instruments', such as the pile or battery, required time and experience to perfect the 'methods of experimenting'. Methods of analysis also required refinement before all data became reliable. Dr. John Davy suggested another reason for his brother's clutchings at alternative explanations, and for his hesitations over interpretations. Other chemists, especially the French chemists Gay-Lussac and Thénard, were busy on the same problems. They had been working while he was ill. Humphry Davy feared, his brother suggested, that his rivals would outstrip him. This fear made him hurried, and hurry made him confused.

If there were hurry, or anxiety to anticipate rivals, there were also genuine difficulties. Oxygen, for example, misled chemists as well as enlightened them. Its prominence in chemistry tempted exposition into relying too much upon it. Since it was a very active element, it was tempting to assume its presence in all vigorous chemical reactions. When bodies burned in oxygen the combustion usually emitted heat and light. All reactions accompanied by heat and light suggested that they were really combustions in which oxygen participated. Many substances emitted heat and light when they united with chlorine, therefore there was oxygen in it. The term *combustion* was finally applied to *all* reactions accompanied by heat and light, whether oxygen were a participant or not.

Davy's ultimate proof that chlorine contained no oxygen considerably assisted this extension of the term 'combustion'. Meanwhile the future 'chlorine' seemed to him to be '*oxy*muriatic acid'. This traditional opinion was too firmly established to be refused, or to be easily refuted. The gas would naturally be vigorous if it contained vigorous oxygen. It was also electro-negative, for it moved to the positive wire under the pile; if it contained oxygen that also behaved so, this was natural.

Hurry neither lodged the preconceived opinion in Davy's mind that chlorine did contain oxygen, nor prevented him, in the course of two years, from dislodging it. Both an insistent tradition and apparent probabilities favoured the preconception. Both Davy's experimental skill and his power to dismiss an inveterate notion finally rejected it. This period of perplexity was effectively preparative, for difficulties and competing alternatives prepared him to perceive, by systematic research, that oxymuriatic acid was presumptively elementary chlorine.

Davy was suspecting oxygen in other substances than oxymuriatic acid. Potash and soda were *alkaline*. So was the 'volatile alkali' (ammonia). Potash and soda contained some *oxygen*. So, therefore, might the volatile alkali. It contained nitrogen and hydrogen. The nitrogen might contain oxygen, and be sufficiently saturated with it to be inert. Hydrogen might hold some oxygen too tightly to part with it, and also be eager for more. These and other sup-

positions, that it would be tedious to relate, attended his somewhat tortuous experimental and speculative course.

Davy was experimenting, and somewhat desperately speculating, his way out of perplexities. He even consulted Ritter once again for light. His battery decomposed water into hydrogen at the negative wire, and oxygen at the positive. Hydrogen might be water combined with positive electricity; oxygen might be water combined with negative. Ritter had reversed the charges. When the two gases united, the two electricities might 'form fire by their attraction or mutual approximation', and the discharged waters be precipitated. The two electricities were regarded as aetherial imponderable principles. They were fluids without weight that charged bodies by combining with them. Such rather wild clutchings at alternatives were, however, to be quietened by the enlightening research on chlorine.

Davy's rapid course of experimental revelation was somewhat slowed during these chaotic years, though this preparative period was not entirely barren of discovery. Curiously enough, this slowing coincided with the acquisition of a more powerful instrument. A 'Voltaic battery', at least four times as powerful as any previously constructed, was supplied by 'the enlightened liberality of the managers of the Royal Institution and its principal members' after Davy's convalescence. The 'munificence of a few indi-

viduals' provided him with a still more powerful battery 'of 2000 plates' not long after. Attempts to decompose 'the metals of the common earths' were inconclusive, and he was much less successful with the anticipated 'amalgams' than with the 'amalgams of the alkaline earths' that he did now obtain. Neither 'silex' (silica) nor 'alumine' (alumina) nor 'glucine' (containing beryllium) were very amenable to 'electro-chemical processes', nor to the direct attack by potassium, though he had obtained small amounts of silicon without realizing what he had done, but Davy was distinctly successful with boracic acid. A powerful battery drew a product from it, and the power of potassium secured more. The combustible matter (boron) obtained from boracic acid was obviously related to it as sulphur to sulphuric acid, or phosphorus to phosphoric acid. Davy also discovered telluretted hydrogen a little later on, and observed the analogies between tellurium and sulphur. His researches on 'fluoric acid' (hydrogen fluoride) in 1808 were not so fruitful at the moment as they were to be when chlorine had given significant clues.

V

UNDECOMPOUNDED CHLORINE AND THE ELEMENTS OF CHEMICAL PHILOSOPHY

CHLORINE, like nitrous oxide, was frequently rechristened before it received its final name. Scheele called it 'dephlogisticated marine acid' when he prepared it in 1774 because the 'manganese' (pyrolusite) had taken phlogiston from the 'marine acid' (hydrochloric acid). It combined with phlogiston to form the 'marine acid' on this version, and when Davy finally failed to decompose it he remembered that Scheele regarded it as an 'undecompounded body'. A period of vicissitude, however, lay between Scheele's first notion and Davy's last. The composition

marine acid = dephlogisticated marine acid + phlogiston

did not directly become

hydrochloric acid = chlorine + hydrogen.

As the antiphlogistic chemistry steadily replaced Stahlian versions, Lavoisier and his successors presumed oxygen in all acids. Every salt was represented by

OXYD ACID
((metal + oxygen) + (base + oxygen)).

Muriatic acid, as the 'marine acid' was usually called, was assumed to be X + oxygen, where X was an unknown base. It was oxygenated by the 'man-

ganese' into further 'oxygenated muriatic acid', which was X + oxygen + oxygen. The doubled oxygen in this formulation is not intended to be strictly quantitative. When the oxygenated muriatic acid, the future chlorine, acted on a metal, the metal removed the extra oxygen to form an oxyd:

metal + (X + oxygen + oxygen)
= (metal + oxygen) + (X + oxygen).

The oxyd (metal + oxygen) then united with the muriatic acid (X + oxygen) to form a salt that, as above, was

OXYD ACID
((metal + oxygen) + (X + oxygen)).

Berthollet was originally responsible, in 1785, for presuming the oxygenation of muriatic acid into Scheele's gas, and Lavoisier for introducing it into his own system. Berthollet himself noted that oxygenated muriatic acid was not *acid*. This was strange because more oxygen in muriatic acid was expected to make it more acid still, but Lavoisier and his successors accepted the enigma. Berthollet observed the liberation of oxygen, and the regeneration of muriatic acid when a solution of the gas in water was exposed to light. The reaction now represented by $H_2O + Cl_2 = 2HCl + O$ was then simply regarded as

(X + oxygen + oxygen) = (X + oxygen) + oxygen.

In Davy's 'infant speculations' oxygen gas, phos-oxygen, carried its light into the oxygenated muriatic acid, and the future chlorine became 'muriatic phos-

acid'. Soon after this youthful divagation, in Davy's quickly repentant period, the future chlorine settled also into Davy's chemistry as the traditional 'oxygenated muriatic acid'. It bleached vegetable blues instead of reddening them like other acids, and it emitted oxygen under vivid light. The gas was thus presented in Davy's 1802 Syllabus of Lectures. He afterwards used the name 'oxymuriatic acid', as did contemporary chemists.

In 1800, when electricity was being assiduously applied to decompose substances, Dr. Henry applied it to muriatic acid gas (hydrochloric acid). It decomposed into hydrogen and Scheele's gas (chlorine). This did not agree with the composition (X + oxygen) for muriatic acid. Henry simply ascribed the formation of hydrogen to the presence of water. In 1809 Gay-Lussac and Thénard were insisting that muriatic acid (hydrochloric acid) was (hydrogen + (X + oxygen)), where X + oxygen represented oxygenated muriatic acid—the future chlorine. It might possibly be (X + (hydrogen + oxygen)), a combination of the hypothetical X with water. These formulations explained the reactions observed. Hydrogen united with oxymuriatic acid (chlorine), X + oxygen, to form muriatic acid gas (hydrogen + (X + oxygen)). The electric battery, as in Henry's experiment, simply disunited them again. They obtained water by passing muriatic acid over warmed litharge, oxide of lead, but they were unwilling to exclude the traditional oxygen from either muriatic acid or oxy-

muriatic acid. It was reserved for Davy to demonstrate that oxygen could not be extracted from chlorine, nor water from muriatic acid gas, by substances that *contained no oxygen*.

At the end of 1808 Davy still presumed

muriatic acid gas (HCl) = X+water
oxymuriatic acid (Cl) = X+oxygen.

X had not been isolated, and the muriatic acid gas contained from one-third to one-fourth of water. The hydrogen that appeared when potassium acted upon muriatic acid gas would come from the water. Vivid combustions in oxymuriatic acid assured oxygen in it, for oxygen was the great promoter of combustion. Davy tried to remove the oxygen from oxymuriatic acid by burning phosphorus and charcoal in it. Phosphorus reacted vigorously, and he tried to explain the two chlorides that he obtained by the constitutions he was assigning. Charcoal acted less effectively than he hoped, and 'aqueous vapour' might be responsible for the slight action that did occur. Davy, however, did not yet definitely suspect oxymuriatic acid of containing no oxygen.

The impassiveness of oxymuriatic acid or muriatic acid gas towards strongly ignited charcoal did at last rouse Davy's suspicions. If the charcoal was 'previously freed from hydrogen and moisture by intense ignition in vacuo' the impassiveness was complete: electrical ignition of the purified charcoal in the gases had no effect. This was strange if oxymuriatic acid, as was usually supposed, did contain specially 'loose

and active' oxygen. Data were conspiring against this traditional assumption. Oxymuriatic acid and hydrogen united into muriatic acid gas, as Cruickshank had shown; they formed muriatic acid gas without depositing water, as Gay-Lussac and Thenard had demonstrated. Water was always present when hydrogen was procured from oxymuriatic acid with the production of muriatic acid: Gay-Lussac and Thénard had proved this by 'a copious collection of instances'. Davy was observing that the reaction now represented by

$$H_2O+Cl_2 = 2HCl+O$$

was probably responsible for mistaken notions. He was returning to a revision of Scheele's view: muriatic acid simply lost hydrogen, which Scheele called phlogiston, when oxymuriatic acid was produced from it. Muriatic acid was X + hydrogen, and the X was oxymuriatic acid that contained no oxygen. Potassium liberated hydrogen from it by combining with the X.

A critical survey of many data in 1810 revealed 'no experimental evidences of the existence of oxygen in oxymuriatic acid gas'. Nor did vigorous reactions, like those of phosphorus or metals, with oxymuriatic acid compel the assumption. Vivid combustions implied 'intense energy of combination', manifested in heat and light, that was not peculiar to oxygen. The action of muriatic acid gas on litharge was intelligible as

$$(\text{lead}+\text{oxygen})+(\text{hydrogen}+X) = (\text{lead}+X)+(\text{oxygen}+\text{hydrogen}),$$

where X was a *simple* substance, as Scheele had supposed.

The 'ingenious idea of Mr. Dalton' completed the clarification of data. Davy had remarked on Dalton's 'ingenious supposition' to the Royal Society on November 16, 1809. According to this, a proportional number, or *proportion* as Davy usually wrote, represented the combining quantity of each element, or compound. If hydrogen was represented by 1, then, from its relative quantity in water, 7·5 (now 8) represented the proportion of oxygen. This same proportion of oxygen, 7·5, united with 40·5 parts of potassium in potassium oxide. The proportion for potassium was this 40·5. Davy's analytical results were usually a little different from the later and more accurate. A proportion, 32·9 (now 35·5), could be assigned to oxymuriatic acid (Cl). In muriatic acid gas (HCl) 32·9 parts of oxymuriatic acid united with 1 of hydrogen, and in potassium oxymuriate (KCl) with 40·5 parts, the proportion, of potassium. Proportions, 1 for hydrogen, 7·5 for oxygen, 40·5 for potassium, and 32·9 for oxymuriatic acid (Cl), represented the combining quantities of these substances in various compounds. The proportion for potassium, 40·5, united with 16·5 parts of phosphorus, which was again the proportion. In phosphoric acid 16·5 parts of phosphorus, the proportion, united with 22·5 parts of oxygen, with, that is, *three* proportions ($7{\cdot}5 \times 3 = 22{\cdot}5$). Similarly, about 13·5 was the proportion for sulphur. Davy's numbers have been

revised by more accurate analyses, but he had received an important cue from Dalton. The recurrences of a definite proportion in various compounds, either singly or in simple multiple, of a substance like potassium or oxygen or sulphur or phosphorus indicated a *single element*. Sulphur and phosphorus had been troubling Davy; he now perceived them to be undecompounded or elementary substances. The appearance of the proportion 32·9 for oxymuriatic acid (Cl) in many compounds indexed its simple nature. It was an element according to its *proportion*, and ignited charcoal had failed to withdraw oxygen from it.

Davy had now reached an assurance. Oxymuriatic acid was a simple substance. It formed muriatic acid by uniting with hydrogen. When the muriatic acid acted on metallic oxides the reaction

$$(\text{metal}+\text{oxygen})+(X+\text{hydrogen}) \xrightarrow{\text{WATER}} = (\text{metal}+X)+(\text{oxygen}+\text{hydrogen})$$

occurred, where X was *simple*. This X was the real oxymuriatic acid. *Dry* oxymuriatic acid did not bleach, it decomposed water to obtain hydrogen and form muriatic acid, and the liberated oxygen did the bleaching. Its name should be changed to CHLORINE, for it was no longer oxymuriatic acid. The name only denoted its colour, implied no composition, and would embarrass no future discoveries, even if they did disclose that, after all, it was a compound. Davy only insisted that *on*

present evidence chlorine was an element. He insisted here, as he always did insist, on the relativity of presumed elements to analytical achievement. It was always possible that any ostensible element might be decomposed by superior methods.

Some recalcitrants, as usually happens, remained hostile even to this claim for elementary chlorine. John Davy was deputed to reply to the obdurate Murray, who remained unconvinced, and still persisted that both muriatic acid and oxymuriatic acid contained oxygen. During the controversy Humphry Davy discovered 'euchlorine', and John Davy discovered phosgene. The *Edinburgh Review* counselled hesitancy, and required more proofs, but most chemists soon accepted the elementary nature of chlorine. John Davy said that his brother's views were 'generally received and taught in the schools' within two years. This sounds rather rapid. Sir Humphry Davy wrote to his brother John, in a letter from Florence dated March 18, 1814: 'The old theory is nearly abandoned in France. Berthollet, with much candour, has decided in favour of chlorine. I know no chemist but Thenard who upholds it at Paris, and he upholds it feebly, and by this time, probably, has renounced it.' The new version of chlorine did secure fairly rapid favour, though there was also casual recalcitrancy.

In 1818 *Sir* Humphry Davy, as he then was, finally ended his share in the long discussion over chlorine. Experimenters in Scotland, the home of the obdurate

Murray, were obtaining *water* from muriatic acid (HCl) and metals. The oxygen of the water must have come from the acid. Davy explained their error by 'the *oxide* of lead in the glass' or by a smaller quantity of alkali in it. These oxides had supplied the oxygen that appeared in the water. He also passed chlorine, dried by 'muriate of lime' ($CaCl_2$), on to iron wire in a red-hot glass tube. The 'quantity of the first compound of chlorine with iron' formed was 'exactly the same as that produced by the action of muriatic acid gas upon iron'. *No moisture* dewed the end of the tube, though it was kept cool, nor was the slightest appearance of it perceptible. No oxygen was even apparently obtained from chlorine if it was effectively excluded from without. Davy had added another proof to his demonstration that chlorine contained no oxygen.

Davy was knighted on April 8, 1812, gave a farewell lecture at the Royal Institution the day after, and was married two days after that. Though his marriage with Mrs. Apreece did not break his habit of research, it did divide, or mark the division of, his life into two parts. The Royal Institution, however, succeeded in retaining Sir Humphry Davy's services. He accepted the offices of Professor of Chemistry and Director of the Laboratory and Mineralogical Collection. These offices were to carry no salary, nor to involve any lectures. Sir Humphry Davy retained liberty of action, and the Royal Institution retained a valuable adviser.

Sir Humphry Davy was now at the height of his fame. In 1810 the Dublin Society had secured from him 'a course of lectures to a crowded auditory'. In 1811 they had again been favoured with 'two excellent courses of lectures in chemical and geological science'. Auditors thronged the 'laboratory in Dublin', where the lectures were delivered, even more than they crowded the theatre of the Royal Institution. The laboratory was enlarged to 'hold 550 people', but it was still too small. Two guineas were charged for a course, but from ten to twenty guineas were offered by disappointed people for a single ticket. Sir Humphry's Irish audiences, and Sir Humphry's Irish fees, 400 guineas for the first visit and 750 pounds for the second, were an index to his fame. In the fervour of the moment 'the degree of LL.D. was conferred on him by Trinity College, Dublin'. His career of discovery and popularity had been recognized by a knighthood. In this same dividing year of his life when he was married, knighted, and made an Honorary Professor of the Royal Institution, he published his *Elements of Chemical Philosophy* very soon after his marriage.

When Sir Humphry Davy accused Dr. Joseph Priestley of frequent 'hasty composition' he had not, perhaps, taken the beam out of his own eye. The *Elements of Chemical Philosophy* was 'rapidly composed', and presented 'no inconsiderable marks of haste' to the contemporary eye of Young. The biographical brother did not detect any 'material marks

of hastiness, nor of carelessness, nor of any want of systematic arrangement, nor of due keeping and proportion of its parts', but his eye was always sharper for his brother's merits than for his faults. The work was doubtless 'in much estimation when it appeared', as Dr. John Davy said, for Sir Humphry could hardly write without authority, nor without deference, nor without a public, and Dr. Paris rightly 'considered' it 'as one of high importance to the cause of science'. Most critics, however, will agree with Thorpe that it exhibits 'haste and imperfection'. This may explain why Dr. John Davy noted in 1836 that 'a second edition of it has not been required', for the admiring brother was more commendatory than discerning when he wrote: 'though rapidly composed, it was not, in fact, hurried'. The *Elements of Chemical Philosophy* was an important work in spite of its frequently hurried writing. Sir Humphry Davy could expand some of his opinions in its pages without being confined by 'the studied compression and elaborate brevity' of the 'Philosophical Memoir', as Dr. Paris said. He could also present his discoveries in a systematic manner—these had been formerly distributed piecemeal in scattered papers. It was a landmark of the progress of science in its day, as it still is, and it still gives an important glimpse into the mind of Davy, as well as into the science of the day.

Lavoisier's discoveries had made oxygen predominant and central. In the *Elements of Chemical*

Philosophy another element, strongly electro-negative like oxygen, and also a vigorous supporter of combustion, shared its central role. This element was chlorine. The discovery of the elementary nature of chlorine handled some old conceptions severely. At least one other substance than oxygen could react vigorously with the evolution of heat and light. At least one acid, muriatic acid, a compound of chlorine with hydrogen, contained *no oxygen*. The notion of *combustion* was now fairly free to extend, though the significance of the acid without oxygen was to require time to appreciate. Curiously enough, Davy's disclosure of presumptively elementary chlorine was one item in an embarrassment.

Lavoisier's revolution had suddenly swollen the list of presumed elements from a mere handful to thirty-two. Lavoisier provisionally included imponderable, or weightless, caloric and light among the elements. The four imponderables, caloric, light, electricity, and, subsequently, magnetism, were contemplated in many chemical lists for many years. The ordinary ponderable elements themselves were becoming a formidable array in 1812. Davy could record forty-two admittedly simple bodies in 1802, and his own researches speeded the steady tendency to swell the list. There is a very perceptible uneasiness over the steadily increasing number of the elements in the *Elements of Chemical Philosophy*. It seemed to Davy that there were too many elements.

A single primary matter had constantly allured

speculation. Sir Humphry Davy noted in 1812 that the 'forms of natural bodies' might 'depend upon different arrangements of the *same* particles': each presumed element might be a very intact, but not finally indestructible, disposition of the uniformly similar particles of a primary matter. This had been 'a favourite hypothesis' in the 'earliest era of physical research', and had often been 'supported by the reasonings of the ablest philosophers'. If this were so, no ostensible element need be genuinely indestructible, for it could be, in principle and possibly in fact, resolved into its primary particles of the primary matter.

Davy's uneasiness is visible in his warnings against assuming that 'any real indestructible principle' had been discovered. It is visible in his suggestion that two or three simple substances might be sufficient to diversify the material world by their numerous combinations. It is visible in lingerings over possible dispositions of one primary element in many apparent principles. Uneasiness over the number of the elements, and hesitancies over admitting them to be genuinely simple or primary, sharpened his eye to perceive a possible primary element among those already apparently disclosed.

Hydrogen, Sir Humphry Davy suggested, seemed to be more like a genuine element than any other substance. Its parts repelled one another strongly, it attracted the particles of other substances, and it combined very energetically with other bodies. It

also, he added, entered into combination in smaller quantities than any other substance. Shortly after, in 1815–16, Prout definitely suggested that hydrogen actually was the primary matter. This might well be so if the atomic weights of other elements were simple multiples of the atomic weight of hydrogen. It is quite probable that Prout secured his first hint from Sir Humphry Davy's suggestion. Prout incidentally remarked in his second paper, in 1816, that the notion was 'not altogether new'. Davy's suggestion of 1812 would not be 'altogether new' in 1816. The *Elements of Chemical Philosophy* would almost certainly be known to Prout. Sir Humphry Davy was too eminent, too prominently before the public, and his *Elements of Chemical Philosophy* then too well known, for his surmised identification of hydrogen with the primary matter to be unfamiliar to Natural Philosophers in England. The identification would probably be discussed in unrecorded conversations. Prout mentioned the anticipation of his theory incidentally enough to suggest that it was familiar. It would almost certainly be familiar if it occurred in the *Elements of Chemical Philosophy*, and it did occur there.

Dr. John Davy considered the *Elements of Chemical Philosophy* to be more 'an epitome' of Sir Humphry Davy's 'own labours, and discoveries, and peculiar views' than its title indicated. Sir Humphry often prescribed opinions for chemistry to accept. He often anticipated changes of opinion by changing

his own, and he often made discoveries to compel the opinions to change. He was often prompt in changing where chemists as a body were more reluctant. The *Elements of Chemical Philosophy* records one important change of opinion, among others, that Davy made more promptly than many chemists.

In 1800, when Davy had just abandoned his youthful 'phosoxygen' speculations, oxygen and nitrogen still seemed to him to be *combined* in the atmosphere. This was the most usual opinion at the time. It was difficult to explain the very uniform spreading of the two gases through one another if there were no combination. The heavier gas, oxygen, would tend to settle downwards, and the lighter gas, nitrogen, would tend to rise upwards, if the two 'elastic fluids', as gases were then often called, were only *mixed*. Did not mercury drop below water, and water settle above mercury! The difficulty was still greater with the still heavier gas carbonic acid. When Davy thought, in his first essays, that plants had to be busy to prevent carbonic acid from settling on the earth like a blanket he shared a notion of his day. It was quite usual to assume *mild combinations* between all the atmospheric gases—including water-vapour. These mild combinations drew the gases through one another, and mixed them well. In 1800 'the equable diffusion of oxygen and nitrogen through every part of the atmosphere' seemed to him to imply the operation of affinity between them. He had other reasons at the time for this belief.

An erroneous composition for air, derived from the authority of Lavoisier, misled him. The density of air was *less* than calculation required for a mixture of twenty-seven parts of oxygen and seventy-three parts of nitrogen. The two gases seemed to *expand* on mixing, and this implied *combination*. Another error also misled Davy: 'atmospheric air' appeared to dissolve 'undecompounded in water'. If water did dissolve nitrogen and oxygen out of the air in the *same proportions* as those in which they existed in the atmosphere this again suggested combination. Davy also observed that ignition converted 'nitrous oxide' into 'nitrous gas and a gas analogous to common air'. A review of these facts seemed to him to make chemical union between the two constituents of the atmosphere 'almost demonstrable'.

The two analytical errors had been corrected in 1807. Lavoisier's mistaken composition for air had been superseded, and a Committee of the French Institute had conformed their estimations to Davy's 21 per cent. by volume of oxygen. Since oxygen, nitrogen, and carbonic acid could be mixed in the proper proportions to compose an 'elastic fluid, having all the properties of air', without any change of volume, the atmospheric gases were mechanically *mixed*. Since distilled water dissolved proportionately more oxygen than nitrogen from air, the gases could hardly be 'chemically combined'. No change of property occurred, nor was there any change of volume, when oxygen and nitrogen formed the air,

or an artificial imitation of it. The atmospheric gases were mechanically mixed because these phenomena were absent. Davy did not presume *mild* combinations, as Berthollet suggested and other chemists also adopted, that occurred without the manifest signs of vigorous chemical union. This change of opinion involved a further perplexity. The atmospheric gases were certainly effectively mixed, though their differing densities tended to settle them into layers. Davy had to find the stirring-stick that mixed the atmospheric gases. He found it in atmospheric turbulence.

A vivid sense of this atmospheric turbulence pervades the relevant passages of his *Elements of Chemical Philosophy*. If the air were visible it would present a more violent tumult than the sea in the fiercest tempest. Tides, currents, and the obvious sweep of winds, constantly disturbed it. Tremors of the earth shook the air into motion, and its constituent gases into mixture. The gases of the atmosphere were constantly shaken through one another as oil and water can be shaken through one another in a bottle. The bottle comes to rest, but atmospheric turbulence never ceases. This appeal to atmospheric turbulence was long a mainstay of exposition as the mechanical mixing of the atmospheric gases was reluctantly admitted. In 1833, four years after Davy's death, when Graham had discovered the law of the diffusion of gases, the 'corpuscular philosophy of the day' could not interpret the fact of gaseous diffusion. In Davy's

expositions, as in many expositions after him, a tumultuous atmosphere relieved speculative difficulties. Then, about the middle of the century, kinetic versions of gases softened the enigmas of gaseous diffusion.

The *Elements of Chemical Philosophy* looked both backwards and forwards. Old ideas, not yet decisively rejected, lingered in its pages. Hints at future revelations also pervaded them. The ghost of phlogiston was not yet fully laid. An 'order' of 'natural resemblances' included potassium, platinum, and other metals in 'one chain of natural bodies'. Ammonium, if it were a metal, as its presence in a genuine 'amalgam' with mercury seemed to imply, would be one of the 'chain'. Since it certainly contained hydrogen, all the other metals might contain it too. This revised phlogistian version was an alternative too dear to Davy for it to receive no mention in his treatise. It was now a lingering relic of old notions. Davy's preface, and references in his text, hinted at a future discovery. He had disclosed the vigorous element chlorine in the muriates (chlorides) of potassium and calcium. He suspected another even more vigorous member of the chlorine family in the fluate (fluoride) of calcium, and in other fluates. He was soon to confirm his suspicion.

VI

EIGHT YEARS OF RESEARCH

WHEN Davy laid down his pen in 1812 it was not long idle, for he never left 'nothing in his inkhorn', nor did he forsake the laboratory where his most intense interests were active 'amongst the objects of scientific research'. On October 14, 1813, he was just starting for 'the Continent upon a journey of scientific inquiry'. Foreign laboratories were to be open to him, but he carried a portable laboratory with him, as he had previously done on his 'tour of pleasure' in Scotland with Lady Davy. The 'commodious portable apparatus', as John Davy called it, was destined to assist in discoveries. So was an 'assistant in experiments and in writing' whom Sir Humphry Davy took with him. This assistant was Faraday, who was now participating in the long record of achievement begun by Davy at the Royal Institution, and destined to continue it. Davy could not refrain from discoveries, and before his tour began he had completed 'a triad of supporters of combustion'. He had also been busy with 'a new detonating compound' discovered by Dulong. Davy injured an eye, though its sight was fortunately restored, in identifying the new substance with 'a compound of azote and chlorine'. The violently explosive nitrogen chloride, as it is now called, hindered his researches, but it did not stop them.

The 'triad' consisted of oxygen, chlorine, and fluorine. The first two were known in 1812, the third was only suspected. Davy had confirmed the suspected fluorine before he left for the Continent, though he never succeeded in isolating it. Oxygen and chlorine were closely associated in Davy's mind, and classed together in his system. Both were presumptive elements because they had not been 'decompounded'; the galvanic current separated each at the 'positive surface', so that both were, in Davy's estimation, electro-negative; both combined vigorously with inflammables; chlorine also formed an *acid* with hydrogen, just as oxygen formed acids in many of its combinations. Oxygen and chlorine were credited, in the *Elements of Chemical Philosophy*, in 1812, with similar 'electrical relations' and like 'chemical powers'. Davy now confirmed his previous suspicions that the fluates (fluorides) were 'a peculiar class of bodies', just as the muriates (chlorides) were, and that they contained a 'peculiar principle analogous to oxygen and chlorine'.

The intense activity of fluorine compelled Sir Humphry Davy to be content with deducing its existence in combinations. It was too active to be confined for inspection in a bottle or vessel. Van Helmont's Alkahest had excited the seventeenth century by its reputation for turning all things into water. Kunkel asked how it could be held in the bottle. This gibe was constantly repeated before Nicholson again recorded it in his *Dictionary*. The

gibe was inapt, for the Alkahest had to be appropriately applied before it would transmute. The third supporter of combustion spoiled the bottle without waiting for appropriate applications: in Davy's metaphor it resembled the 'fabled waters of the Styx' that could not 'be preserved'. Davy suspected 'pure liquid fluoric acid' (HF) of containing a 'peculiar undecompounded principle, analogous to chlorine and oxygen', combined with hydrogen. The 'liquid fluoric acid' itself ate into glass bottles, but it could be contained in vessels of horn-silver (AgCl) or platina. It could then be subjected to the galvanic current. 'Voltaic electrization' seemed to decompose it into hydrogen and a furiously active substance that attacked its container too vigorously to be kept for inspection. Davy tried to expel the fluorine from, for example, the 'fluates (fluorides) of potassa and soda' by treating them with chlorine. It seemed to be expelled, but it attacked surrounding substances too quickly to be studied. On one occasion a 'peculiar smell' apparently preceded its rapid attack on its surroundings after its disengagement by chlorine. Davy suspected that he had smelt fluorine during its fleeting moment of freedom. He also sought help from the 'proportions'. If fluor spar contained calcium and fluorine, and the number for calcium were 40, the number representing the new substance would be 34·2. Experiments on fluates of potassa and soda disclosed 33 as the probable proportion for fluorine. Its compound with hydrogen seemed to

indicate a similar proportion. Davy was deriving help from these considerations, though his proportion for fluorine was well over double its present atomic weight. He called the deduced element 'fluorine', as Ampère suggested to him. He also suspected that 'other more subtile bodies belonging to this class' of undecompounded bodies would be discovered. *Iodine* was to confirm this suspicion not long after.

Iodine was as willing to appear in violet vapour or lustrous crystals as fluorine was in a hurry to vanish. A paper on *Some Experiments and Observations on a New Substance which becomes a Violet-coloured Gas by Heat*, dated from Paris on December 10, 1813, and read before the Royal Society on January 20, 1814, intimated that Davy's continental journey was to be a tour of discovery. Courtois, a Paris manufacturer of saltpetre, had prepared the violet vapour by the action of sulphuric acid on ashes of seaweeds, about two years before, and condensed it into lustrous crystals. French chemists had been busy with the new substance. Davy was elected a Corresponding Member of the First Class of the Institute on December 13, 1813, and Ampère gave him a little *X*, as iodine was then called, in the previous November. The gift speeded the inquiry, as John Davy noted with brotherly admiration. It also, unfortunately, resulted in anger against Ampère for giving Davy the substance, and against Clement for asking him to analyse it. Davy, with characteristic

insight and rapidity, quickly resolved riddles that were perplexing or misleading others. Clement apparently regarded the compound of *X* with hydrogen as muriatic acid (HCl) until Davy showed that it was, in his first terminology, hydroionic acid (afterwards hydriodic acid). When Davy had quickly suspected the analogy between iodine and chlorine, and rapidly followed the clue, the French chemists welcomed the clarification without so heartily welcoming the source. Sir Humphry Davy was too personally unpopular in Paris, though his genius was recognized, for his brilliant sally into the research to be wholly welcome. He was prepared by experience and expectation to recognize the family resemblance between *X* and chlorine, but he had surprisingly outlined the chemistry of iodine from a small quantity of material. Davy considered that he had been empowered to communicate his results to the Royal Society, and the Society considered it right to publish them. Davy realized that iodine was less strongly electro-negative than chlorine: it had, as he wrote, 'positive energy with respect to chlorine'. This was evident from the fact that galvanism separated from the compound of the two elements the iodine at the negative wire. Davy continued the researches on iodine at Florence, and the Royal Society listened to the results on June 16, 1814.

The chemistry of the diamond had preoccupied Davy's mind considerably. It was so perplexingly unlike charcoal or plumbago if it were only com-

posed of carbon. During his period of perplexity, between 1808 and 1810, he had suspected it of containing a little oxygen. In his *Elements of Chemical Philosophy*, 1812, such a trace of oxygen did not 'harmonize with the doctrine of definite proportions': there was too little oxygen to correspond to its 'proportion'. If the diamond were 'ultimately found' to be 'merely pure carbon' it would be one argument for differently arranged primary particles in apparently elementary forms of matter. It still seemed to him in 1813, however, that there must be 'some chemical difference' between charcoal and the diamond. It seemed contrary to 'perfect chemistry' or 'analogies of nature' that totally different physical properties should be associated with identical chemical compositions. The mistakes over iodine now presented a possible parallel. The compound of iodine with hydrogen had been confounded with muriatic acid; a peculiar matter in the diamond might be similarly responsible for its apparent chemical identity with charcoal. Davy may have been surmising that as (iodine + hydrogen) appeared to be (chlorine + hydrogen), so (X + oxygen) appeared to be carbonic acid (carbon + oxygen), to which charcoal burnt, if X were the peculiar matter of the diamond. He did not, however, incline to deny that the diamond contained carbonaceous matter, but it might also contain 'an extremely light and subtile principle'. He had suggested, before he left England, that subtle supporters of combustion might await

discovery, and that the 'light and subtile principle' might escape from the burning diamond, or be newly combined. His experiences with iodine strengthened the suggestion that an analogue of oxygen, chlorine, and fluorine might lurk in the diamond. The 'great lens in the Cabinet of Natural History' at Florence tempted him to experiment with it upon the gem. It had been used 'in the first trials on the action of solar heat on the diamond, instituted by Cosmo III, Grand Duke of Tuscany'. Davy now used it for further trials, before undertaking 'researches on the combustion of different kinds of charcoal at Rome'. He reported to the Royal Society, in a paper read to it on June 23, 1814, that 'the purest charcoal' only differed from the diamond by containing 'a minute proportion of hydrogen'. It seemed to Davy that so small a constituent, 'less in some cases than $\frac{1}{500000}$ part of the weight of the substance', could hardly produce sensible differences in character. He himself had speculated on the origination of different properties in different arrangements of like particles. Tennant referred the difference between the diamond and charcoal to *crystallization*. It now seemed to Davy that the diamond was probably crystallized carbon.

When Davy returned to London on April 23, 1815, the Royal Society had received further news of researches during his travels. The topics were the *Colours used in Painting by the Ancients*, a *Solid Compound of Iodine and Oxygen*, and the *Action of Acids on*

Hyper-oxymuriates. Another problem soon received a solution by his rapid effectiveness.

The *safety-lamp* represented both Sir Humphry Davy's discerning rapidity in solving an experimental problem and his unselfish scientific devotion to the service of humanity. He faced his new problem for the first time on August 15, 1815. A letter from the Reverend Dr. Gray directed his attention to guarding coal-miners from explosions, and he began the experimental investigation soon after. The principle of the safety-lamp was announced to the Royal Society on November 9, and the lamp itself was perfected in December. By the first of June, 1816, as Mr. John Buddle then informed Sir Humphry, in a letter from Wallsend Colliery, Newcastle, 'the Wire-gauze Safe-lamp' had been used 'daily in every variety of explosive mixture for upwards of three months'. The colliers had been convinced, and eagerly adopted the lamps. About three months before, on March 18, 1816, a vote of thanks from a general meeting of coal-owners at Newcastle intimated that Davy's experimental powers had survived the stringent test of the mine. Obvious success had confirmed his discernment before; a striking practical achievement confirmed it decisively again. Sir Humphry Davy refused to put 'four horses' to his 'carriage' by patenting his invention. He had intended to protect men from death or mutilation; he was satisfied to have succeeded without also protecting the protection. A moment of controversy

urged the prior claim of George Stephenson to the invention, but a banquet and gift of plate from the coal-owners, on September 25, 1817, finally expressed the gratitude of the coal-mining community to Sir Humphry Davy for his application of science to save life and promote industry. Science marches, invention marches with it, and the modern safety-lamp has superseded Davy's, but the 'wire-gauze safe-lamp' is a permanent memento of a step in science, and of Davy's contribution to it. The Royal Society honoured him with a Rumford medal; the nation added *Bart.* to his name on October 20, 1818.

Davy simply enclosed the flame of the lamp in a cage of wire-gauze. The mixture of air and 'fire-damp' within the lamp burned quietly without transmitting its flame to travel as an explosion in the mixture without. The 'flame sieve' of metal permitted gas, air, or light, to pass, but it cooled the gaseous mixture down so that the flame could not travel through it. A current or wind might mingle the explosive mixture of fire-damp and air into the flame within the lamp too rapidly for the gauze to act, so Davy included among a few additional conveniences a screen to stop such draughts.

Davy confirmed the identity of coal-mine fire-damp with the 'inflammable gas of marshes'. Dalton had demonstrated its 'exact chemical nature', and it contained '4 proportions of hydrogen in weight 4' to '1 proportion of charcoal in weight 11·5'. Davy defined flame as 'aeriform, or gaseous matter, heated

to such a degree as to be luminous'. Nicholson's *Dictionary* said that 'flame always consists of volatile inflammable matter in the act of combustion and combination with the vital part of the atmosphere'. It was usually, Davy now remarked, 'a continued combustion of explosive mixtures', but chemical change was not essential, as the glow of 'an undecomposable gas' under the 'discharge of voltaic electricity' demonstrated. Attempts to pass 'flame through a very fine mesh of wire-gauze at the common temperature' were hindered by the cooling action of the gauze on 'each portion of elastic (gaseous) matter' passing through it. This could reduce the temperature below the luminous point—below the temperature at which the combustion could continue. The effectiveness of the cooling reduction was the greater, the less the heat produced by the combustion, the thinner the wire, and the finer the mesh. Since fire-damp was very fortunately 'the least combustible of the known inflammable gases', a 'coarse texture and wire' was sufficient for the lamp.

In 1816 Sir Humphry Davy had a little skirmish with Gay-Lussac over the relationships between some of the elements—'undecompounded substances' as Davy usually called them to avoid the dogmatic implication of 'elements'. Chlorine and iodine seemed to Gay-Lussac to be less like oxygen than like sulphur. They still seemed to Davy to be more like oxygen. These surveys contain hints of the future,

but the adequate grouping of the elements required more knowledge, and many more years, before it could be secured.

Davy's 1816 paper contained one very significant hint at a future subversion of a traditional doctrine. Since Lavoisier oxygen had been established as the *acidifying principle*. The traditional acid necessarily contained oxygen united with an acidifiable substance or base. The acids themselves were these oxygenated compounds—sulphuric acid being oxygen and sulphur, for example, or nitric acid being nitrogen and oxygen. Chlorine, so long as it was oxymuriatic acid, did not breach this tradition, nor preclude muriatic acid (HCl) from its inevitable oxygen. Undecompounded chlorine uniting with hydrogen to form a strong acid did breach it, and Davy promptly suspected it of being an acidifying principle as well as oxygen. Iodine breached the tradition analogously. In 1814 Gay-Lussac suggested the abandonment of the Lavoisierian limitation to oxygen-acids. Both chlorine and iodine formed acids either with oxygen or hydrogen. There were, therefore, hydrogen-acids as well as oxygen-acids. Sulphuretted hydrogen had been constantly suspected of containing some oxygen because it was feebly acid. Davy had finally surrendered the presumed oxygen in sulphur. Thus sulphuric acid was an oxygen-acid of sulphur, and hydrosulphuric acid, as Gay-Lussac called sulphuretted hydrogen, a hydrogen-acid of it. In 1815 Gay-Lussac also realized that prussic acid was also a hydracid.

He had prepared anhydrous prussic acid in 1811 by distilling its mercury salt with concentrated muriatic acid (HCl), and successfully analysed it in 1815. In 1816 Faraday procured the acid by Gay-Lussac's method for Davy to examine. Davy satisfied himself that it contained *no oxygen*. Then he hinted very distinctly that *all* acids might be hydrogen-acids. 'An acid composed of five proportions of oxygen and one of nitrogen is altogether hypothetical,' he remarked during a discussion of Gay-Lussac's notions in 1816. It was 'a simple statement of facts to say that liquid nitric acid is a compound of two proportions of hydrogen, one of azote, and six of oxygen'. Davy died many years before chemists realized that all acids *were* hydracids, or hydrogen-acids: that sulphuric acid, for example, was H_2SO_4, and not SO_3. Dumas remembered in 1836, when the hydrogen-acid version was becoming more urgent, that Davy had anticipated it. Davy pointed to the fluates (fluorides) and muriates (chlorides). These neutral salts did not contain the acids nor alkalies from which they were formed. Nitric acid and sulphuric acid could not be regenerated from nitrates and sulphates 'without the intervention of bodies containing *hydrogen*'. When muriatic acid acted on lead oxide water was formed from the oxygen of the oxide and the hydrogen of the acid. Analogy suggested that the corresponding water in similar reactions with other acids, even if these did contain oxygen, came from the *hydrogen* of the acids.

Davy, however, apparently did not press the hydrogen-acid version on his contemporaries. He frequently surveyed alternative notions without necessarily adopting them, as he surveyed revised phlogistianism without adopting it. He did not press the hydrogen-acid version in his *New Experiments on the Combinations of Phosphorus* in 1818. Dulong, as Dumas also remembered, hinted also at a hydrogen-acid theory. When Dulong 'advanced the ingenious opinion, that the hydrophosphorous acid may be considered as a triple compound of hydrogen, oxygen, and phosphorus', Davy did not compare it with his own previous version of nitric acid as a triple compound of hydrogen, oxygen, and nitrogen, but suggested, alternatively, 'that it may be a compound of phosphoric acid and phosphuretted hydrogen'. He also wrote of 'hydrated phosphoric acid'. He was almost certain that 'the phosphorous acid is a direct combination of phosphorus and oxygen'. He also suggested that future experiments might decisively assign to hypophosphorous acid a *binary* constitution of oxygen and phosphorus. If Davy was not simply conceding to tradition for expositional clarity, as was done long afterwards by definite advocates of the hydrogen-acid version, he did not press the inclusion of all acids among the hydracids. He had hinted anticipatively at a future change of opinion, but he did not, apparently, positively adopt his own hint into an actual anticipation.

Sir Humphry Davy, Bart., travelled to the Conti-

nent again in 1818 to explain his safety-lamp abroad, and study the 'Papyri found in the Ruins of Herculaneum'. His brother suspected reports of volcanic activity observed in the winter of 1819 and 1820 in a 'description of a volcano in activity' in 'one of his unfinished dialogues'. Sir Joseph Banks, the President of the Royal Society, died on June 19, 1820, just after Davy's return to England. Davy probably hastened back because he was anxious for the office about to become vacant. Though Sir Joseph Banks had recommended Dr. Wollaston as his successor, the Society appointed Sir Humphry Davy to the Presidential Chair, and re-elected him for seven successive years.

VII

FROM 1820 TO 1826

SIGNIFICANT experiments, from time to time, are rapidly and abundantly fertile in consequences. One such significant experiment concurred with Davy's first step to fame when Nicholson and Carlisle decomposed water in 1800. A second concurred with his final step into the Presidential Chair of the Royal Society in 1820. In that year Oersted placed a wire parallel to a pivoted magnetic needle. When he passed a galvanic current through the wire the needle turned through an angle. When the current was reversed, the needle turned in the opposite direction. In July 1820 Oersted publicly announced

that, in the words of Dr. John Davy, 'when the extremities of a voltaic pile or battery are united by a perfect conductor, as a metallic wire, and the compass is brought near it, the needle is attracted by the wire, and may be made to deviate from its natural direction'. A hint of this discovery in a friend's letter from Geneva kept Davy from Cornwall at the table where he was 'magnetising'. The letter was apparently not very explicit, for Davy referred to the 'vague experiments of Oersted's'. The 'Danish philosopher' was certainly not vague in his own accounts, but Davy had discovered for himself by October 19 that 'the voltaic pile is a powerful magnet'. The Royal Society was soon listening to a record of his experiments and inferences on November 16, 1820.

It seemed to Davy that one significant experiment had interfered with another. Natural Philosophers had been too busy decomposing substances by 'the wonderful combination of Volta' to apply it to the compass needle. The chemical and electrical performances of the pile, or battery, had concealed its magnetic powers. Davy deduced that 'the wire itself became magnetic during the passage of the electricity'—iron filings, for example, behaved to it as if it were a magnet. The 'magnetism produced by voltaic electricity' seemed to be in 'exactly the same ratio as the heat', for a 'fine platinum wire' was most powerfully magnetic when it was 'near the point of fusion' under a strong current. Needles placed trans-

versely to a wire through which 'the electricity of a common Leyden battery was discharged' became magnets, just as they did 'in the voltaic circuit', and 'according to precisely the same laws'. Ampère was to be the great immediate reaper of the harvest sown by Oersted, as Davy had been the great reaper from the experiment of Nicholson and Carlisle, but Oersted's discovery quickly prompted Davy to some experimental steps. A body magnetized by electricity, like the wire in the original experiment, could move a magnet. He discovered that, inversely, a magnet could move the body magnetized by electricity. The laboratory experiments suggested to him cosmic applications. If 'strong electrical currents' followed the 'apparent course of the sun', the earth would be a magnet, and with its magnetic poles disposed as they were found to be. This penultimate conjecture in Davy's paper, which was written as a letter to Dr. Wollaston, was succeeded by a practical suggestion. If horse-shoes or bars of steel were placed across 'electrical conductors of buildings' the lightning would magnetize them. 'Elevated and exposed situations' should be chosen. When Davy failed to effect chemical changes by magnetism he decided against 'the identity of electricity and magnetism'. He was also unable to affect 'electrified wires by the magnetism of the earth'.

Wollaston tried to make a wire frame through which a current was passing rotate round its axis by approaching a magnet, and Faraday succeeded on

Christmas morning in 1821. Davy threw mercury into 'vortices' by similar methods, and was still more successful in 1823 'immediately after Mr. Faraday had published his ingenious experiments on electro-magnetic rotation'. In a paper read before the Royal Society on July 5, 1821, Davy described magnetic effects upon the electric arc. He obtained 'an arc, or column of electrical light', by connecting two carbon rods, with their free points separated, to 'the poles' of the 'great battery of the London Institution'. He varied his experiments by varying the distances between the separated carbon points in a varying atmosphere. The 'column of electrical light' between the points varied 'in length from one to four inches according to the state of rarefaction of the atmosphere in which it was produced'. A 'powerful magnet', appropriately presented, imparted 'a rotatory motion' of attraction or repulsion to the 'arc or column'. These, and other experiments, were one interesting interlude in Davy's long programme of research, but he did not rival Ampère here, nor did his name become so intimately associated as Faraday's with electro-magnetism.

History constantly repeats similar situations, though it repeats them differently. A new science of electro-dynamics grew rapidly out of Oersted's discovery, as a new science of electro-chemistry sprang up, like Jonah's gourd, from the experiment by Nicholson and Carlisle. There were many speculations after the event of 1820, as there had been

many after the event of 1800. Davy was again entering warnings against speculative licence, as he had entered them before. He warned his contemporaries against confusing hypotheses, which were easy to frame, with theories, which were more difficult to secure. The distinction between 'hypothesis' and 'theory' has been differently expressed. Dr. John Davy explained his brother's distinction by contrasting Sir Humphry's *hypothesis* that fluorine existed with his *theory* that chlorine was an element. When John Davy, who often elucidated his brother in footnotes, saw the warning against confusing hypothesis with theory in a paper read before the Royal Society on March 6, 1823, he saw another opportunity. He chose the contrast between two of Sir Humphry Davy's own notions to point the contrast intended between hypothesis and theory. The existence of fluorine was a reasonable assumption from relevant data, but it was hypothetical because no experiment had yet isolated the supposititious gas. The theory that chlorine was simple expressed the experimental fact that it behaved as if it were. Chlorine was theoretically an element because it had not been decomposed, and the theory simply expressed many experimental facts regarding it. Sir Humphry Davy was warning his auditors against crediting suppositions or opinions that were merely hypotheses, and exhorting them to ensure progress by 'new facts and experiments' that could be finally expressed in a 'satisfactory theory'.

The word 'analogy' frequently left Davy's lips, and often dropped from his pen. Analogies always seemed to him to be the stepping-stones of thought. In his *Syllabus of a Course of Lectures* for 1802 he noted that light seemed to behave in many ways like 'undulatory motions', and might really be such motions in an aetherial medium. Sound travelled as a propagated disturbance through the air; at that time it seemed to Dr. Thomas Young that light might be an analogously propagated disturbance through the aether. Young altered his model in 1818, when he compared light to the undulation travelling along a rope shaken at its end. Young had been appointed Professor of Natural Philosophy at the Royal Institution in the July of 1801. He stayed less than two years, for, though eminent in science, he was feeble in the lecture theatre. It was natural in 1802 for the young Davy to choose the undulatory analogies of light for special comment. Young was professor at the moment, and was opposing the undulatory to the corpuscular version of light. He exclaimed at the 'wonderful corpuscles' of light that were equally swift whether they leaped from the clash between two pebbles or were shot from the sun. That patron of the literary lash, the *Edinburgh Review*, soon exclaimed at Young, and urgently warned its readers off his aether and undulations. Meanwhile the youthful Davy noted the important analogies that Dr. Young had presented between 'the laws of known undulatory motions, and those of light'.

The aether had seemed to be a superfluous speculative luxury to Davy in his first youthful essays. He was speaking more hesitatingly in 1802 when he realized that light was often more like aetherial undulations than a swift stream of corpuscles. In 1827, two years before his death, it seemed to him certain that a matter did move 'in the space between us and the heavenly bodies'. The aether and its undulations then seemed to him to be the very probable transmitters both of light and heat. Davy, however, had always had a lively sense of the perplexities in the problems relating to heat and light.

Undulatory motions, like waves in water or quivers along ropes, could be perceived and studied. Some behaviours of light seemed to resemble such motions. Analogy connected the two sets of facts by supposing light to be actual undulations in an aetherial medium. This theory, or hypothesis, was an adopted analogy. Such adopted analogies were pervasive in science. Davy insisted on their significance in his 1802 *Syllabus*, in his estimate 'Of the General Logic of Science'. He also insisted on the classificatory value of such adopted analogies. The behaviours of light could often be intelligibly expressed, and clearly presented, by presuming them to be aetherial undulations. Davy had learned very rapidly, and very painfully, from his first rashness, not to be too confident in theoretically adopted analogies. He urged in 1800 that 'even experimental research is only a method of approximation to truth', and he never forgot his

maxim. Disciplined speculation could be guided by analogies, but it should not become wedded to them. In his *Elements of Chemical Philosophy*, 1812, he identified the foundations of chemical philosophy with observation, analogy, and experiment. Observations presented facts, analogies connected them, and experiments revealed them. Hypothetically adopted analogies usefully ordered data, or guided experiment to fresh discoveries. They lapsed, in discerning minds, before contradictory data. Analogies, such as those between the behaviour of light and known wave-motions, were not facts. The insistent 'substitution of analogy for fact', he exclaimed in 1816, 'is the bane of chemical philosophy'. It was very usual for 'all new facts', he had urged in 1807, to bring their own 'train of analogies', and cast suspicion on all former conclusions. Science constantly proceeded by constantly revising its hypothetically adopted analogies. When Sir Humphry Davy, Bart. became President of the Royal Society in 1820 he retained his lively sense of the temporariness of scientific hypotheses. In his first address from the Presidential Chair he identified them with 'parts of the scaffolding of the building of science'. This *scaffolding* conception of hypotheses was ever present to his mind. Even theories, like the theoretically elementary nature of chlorine, that successfully expressed all known data, might be superseded. Davy was always sensible that ostensible elements might be finally deposed. Now, when Oersted had prompted specula-

tions to handle discovered data, he urged a disciplined speculation on his contemporaries. Let the final aim be always at theories that expressed the results of extended experiments without unproved assumptions. Let many experiments be made, and let speculation be modest in its claims.

The death of Berthollet in 1822 imposed upon Davy one of his presidential duties of commemorating the famous dead. This 'patriarch of modern chemistry' had been prominent in establishing the 'anti-phlogistic system' of Lavoisier. Davy himself had experienced the continued power of Stahl's phlogiston, though Berthollet had lived to see Lavoisier's assured triumph. The year after, when Sir Humphry had yet some six years to live, the obscure malady that was to prevent him from completing his fifty-first year of life distinctly began to trouble him. Though he had now made his chief discoveries, his researches had not ended, nor did they end until almost the last moments of his life. In 1823 a future line of progressive achievement received from him a moment of impetus. The name of Faraday must be associated with this early step towards the final consummation, many years afterwards, of the liquefaction of air and other permanent gases.

Chlorine had disconcerted old ideas, and introduced new ones to chemists; it now provided a starting-point for new researches. The yellow crystals that separated from its cooled aqueous solution

seemed to Berthollet to be the solidified gas. This was the usual opinion till 1810 when Davy failed to condense pure dry chlorine 'at 40 below 0° of Fahrenheit'. The gas itself had appeared to crystallize at low temperatures because it had been exposed to cold *in the presence of moisture*. The hydrate of chlorine that had simulated a crystallized gas now provided a first step in a series of liquefactions of gases. Faraday's analysis in 1823 corresponded very nearly with '10 proportionals of water to 1 of chlorine'. The crystals liquefied easily with warmth, emitting much gas. Sir Humphry Davy expected that if chlorine hydrate were heated in a *closed* tube, either it would simply melt, or it would decompose with the formation of 'euchlorine and muriatic acid', or the emitted chlorine gas would squeeze itself into a 'condensed state'. Faraday, when Davy requested him to perform the experiment, heated the chlorine hydrate in one limb of a ∧-shaped glass tube, and collected yellow liquid chlorine in the other. On the 5th of March 1823, Dr. Paris found Faraday in the laboratory of the Royal Institution 'engaged in experiments on chlorine and its hydrate in closed tubes'. He rallied Faraday for using 'soiled vessels' because the experimenter's tube contained 'some oily matter'. Faraday, who was surprised to see the oil, filed off the 'sealed end' of the tube. Instantly, to their 'great astonishment, the contents suddenly exploded, and the oily matter vanished'. Dr. Paris left Faraday to repeat his experiment, and mentioned

the occurrence to Sir Humphry Davy after dinner. Davy seemed to Paris to be as surprised as he and Faraday had been. A laconic note from Faraday, 'early on the next morning', informed Dr. Paris that the mysterious *oil* was liquid chlorine. Faraday undoubtedly first achieved the liquefaction of chlorine on this occasion. Dr. Paris thought that he would have discovered the liquefaction without any preliminary hint from Davy, but Dr. John Davy thought that the hint had been essential. Davy himself, allowing muriate of ammonia (ammonium chloride) and sulphuric acid to act upon one another in a similar sealed glass tube, and in one limb of it, 'procured liquid muriatic acid' (HCl) in the other. He passed the continuance of this research on to Faraday, who liquefied a handful of the more easily condensed gases, though it was not until Davy was dead, and Thilorier liquefied carbonic acid in 1835, that he took another decided step by liquefying some of the less readily coerced gases. He himself was dead before later successes effectively condensed the more permanent gases, such as air. Davy was relying more on squeezing than on cooling gases to liquefy them. The self-compressive method of 'slow generation in closed vessels' avoided the smart reversive rise of temperature during 'sudden mechanical compression'. When gases approached the 'point of compression and temperature at which they became vapours' pressure could be assisted by 'artificial cold'. The recognition of the importance of *cold*

subsequently played an important part in experimental successes. The preliminary liquefactions of the more easily condensed gases, meanwhile, were accomplished by compressive methods, and usually in glass tubes. Faraday also often supplemented the heat applied to the limb where the gas was released by applying cold to the limb where the gas was condensed. Davy had hoped to use vapours from liquefied gases instead of steam, without having to boil them like water. If he could press gases easily into liquids and then allow them to evaporate spontaneously, or with slight heating, he might be able to secure an effective motive power. This hope did not mature, but one frustrated intention indirectly resulted in the liquefaction of chlorine, and then of a few other gases. A few preliminary experiments in 1823 did suggest momentarily that results only 'hitherto obtained by a great expenditure of fuel' might be secured from 'the mere difference of temperature between sunshine and shade'. Even Sir Humphry Davy, however, could not wring this desirable consummation from a reluctant nature. The principles of heat engines were then too little understood for such hopes not to arise, but trials effectively quenched them.

A 'Memoir of Sir Humphry Davy, Bart.', in *The Gentleman's Magazine*, just after his death in 1829, was profuse on his investigations to protect the copper sheathing on ships' bottoms from corrosion by sea water. It was his last extended research, and

it received considerable attention in the Memoir. The article quoted copiously from Mr. Davies Gilbert, who had succeeded Davy as President of the Royal Society in 1827, and had handed a Royal Medal to his predecessor at the anniversary meeting. The change of President was an ominous shadow of a coming event, for it was due to Sir Humphry Davy's failing health. Gilbert remembered the 'American-revolution war' when ships were first sheathed with copper. The iron bolts decayed; when bronze bolts were substituted, 'immediately the copper failed'. The Commissioners of the Navy Board had a similar problem for the Royal Society, and Sir Humphry Davy, Bart., who had made so many inquiries before, now inquired into the 'rapid decay of the copper sheathing of His Majesty's ships of war'.

The inquiries occupied him from the latter end of 1823 to the summer of 1826. They were recorded in three papers read before the Royal Society on January 22, 1824, June 17, 1824, and June 9, 1825. Since the prevention of the corrosion of the copper presented itself as an electro-chemical item, the topic of his last Bakerian Lecture on June 8, 1826, was partly determined by these inquiries. He had chosen *On Some Chemical Agencies of Electricity* for the theme of his first Bakerian Lecture on November 20, 1806; he now chose *On the Relations of Electrical and Chemical Changes* for his last. Though the final lecture contained references to his recent researches, its topic

was also otherwise appropriate. Davy could survey the progress of the electro-chemical science with which he had been so intimately associated. He could review experimental disclosures, and present his own matured versions of the relations that connected chemical with electrical changes. This was Davy's last important paper to the Royal Society. The two later communications on Volcanoes and the Torpedo were more minor.

Davy approached the problem of the corroding copper through his theory, or hypothesis, of electro-chemical phenomena. He deduced probabilities, and verified them by experiment. 'I was led to this discovery by principle,' he wrote to his brother John. The discovery can be quite simply explained, and readily understood, before further discussing Davy's versions, or personal expression, of electro-chemical relations.

Preliminary experiments disclosed the joint action of air and sea water on the metal. If *two* contiguous metals, two metals in close contact, were exposed to the corroding sea water, only the *more electro-positive metal* was severely attacked. The *iron* bolts had corroded first, as Gilbert Davies remembered, because they were more electro-positive. A piece of soldered tin, Davy discovered, was attacked, and the 'polished piece of copper' to which it was fastened was not. The more electro-positive tin *protected* the copper by bearing the brunt of the corrosion. The tin was an effective 'protector' if its own surface was 'about

one-twentieth of the surface of the copper'. It was also effective even in 'sea-water slightly acidulous by sulphuric acid'. Small pieces of zinc or iron nails also protected the copper.

On June 17, 1824, Davy could report to the Royal Society on large-scale experiments. Relatively small portions of zinc or iron in close contact with copper sheets protected them from corrosion by the sea of Portsmouth Harbour. The copper sheathing of boats or ships was similarly protected. The zinc or iron corroded, and by corroding saved the copper. Cast-iron 'protectors' were cheapest, most easily procured, and lasted longer.

Unfortunately for Davy the 'protectors' that stayed corrosion of the copper did not prevent fouling. They did not keep weeds off the copper sheathing, nor barnacles, nor insects, nor mineral deposits, so effectively as they preserved the metal. Most ships did not return with so bright a bottom as the Carnebrea Castle, nor as several other protected vessels. The Admiralty removed the 'protectors' from their warships. This decision vexed Davy greatly. The 'protectors' would have been perfectly efficient, Dr. John Davy bitterly remarked, if the bottoms of warships were as carefully cleaned as their decks were holystoned. 'Electro-chemical protection', however, might be applied elsewhere. Davy suggested 'a piece of zinc or tin' for preserving boilers, though a qualifying footnote questioned whether the zinc, which was preferable, were really required, since

iron did not decompose water at 212° F. Davy made this suggestion at the end of his last Bakerian Lecture on June 8, 1826, when he chose *On the Relations of Electrical and Chemical Changes* for his theme, and gave his final summary of the electro-chemical researches with which he had been so intimately associated.

The 'common electricity' from electrical machines, or the 'Leyden Phial', was known before the galvanic current from the 'pile of Volta'. Davy reminded his audience in 1826 that Volta had always maintained 'the identity of the cause of galvanism and electricity'. In 1801 Dr. Wollaston had decomposed water by placing two platinum points in it, and using discharges from an electrical machine instead of a voltaic pile. It was possible to collect the potash from a solution of potassium sulphate round one fine platinum point, and the sulphuric acid round another, by 'common electricity' from 'a powerful electrical machine'. The galvanism of the pile and the electricity from the machine effected the same decomposition. The 'common electricity' from the machine could also transfer sulphuric acid from one vessel 'through moist asbestus' into another containing water, just as the pile could. Davy had secured these results by 1806, and had concluded 'that the principle of action is the same in common and the Voltaic Electricity'. In 1826 the two electricities still seemed to him to be essentially identical, and 'some very decisive experiments' by Dr. Wollaston

seemed to have 'placed the identity of the cause of galvanism and electricity, which had always been maintained by Volta, out of all doubt'.

There was no unanimity over the nature of electricity. Davy noted the diversities of opinion in the fourth edition of his *Elements of Agricultural Chemistry*, published in 1827. Some presumed that vitreous or positive electricity was one fluid, and resinous or negative electricity another fluid. Others preferred the one-fluid to the two-fluid version. Positively charged bodies, according to this, contained an excess of the single fluid, and negatively charged bodies had a deficit. He had noted a predominant preference for a single electrical fluid in 1802. Others rejected both two fluids and one fluid. Electrical effects, for many of the latter, were 'affections or motions of matter, or an exhibition of attractive powers'. Electrical attractions or affections of matter seemed to them to be for 'masses' what the agencies of chemical combinations or decompositions were for particles.

Davy himself, in 1812, had conceived the *same power* to produce 'electrical changes' by acting on masses, and 'chemical changes' by acting on particles. He had thought so before 1812, and he still thought so in 1826. This had always been hypothesis, and hypothesis it still remained, but it was based on copious induction, and it had led to practical results. Davy also preferred a two-fluid version. 'Two etherial, subtile, elastic fluids', repelling their own par-

ticles and attracting one another's, that could combine in different proportions with bodies, might be presumed. Hypothesis, however, could prefer a single electrical fluid, and not be confuted. No 'direct demonstrative evidence' had yet decided between the alternatives, nor upon either.

'The intimate connexion' between 'chemical and electrical phenomena' also seemed to Davy in 1826 to be referable to a controlling principle. Chemical changes always tended to restore disturbed electrical equilibrium. The contacts between metals in fluids, for example, destroyed the electrical equilibrium by charging the metals, and chemical changes in the circuit tried to restore it. The reasoning that led him to discover 'protectors' for copper in tin, or zinc, or iron, seems to have been as follows. Copper combined with oxygen, which was one participant in the corrosion, because this metal was naturally electro-positive, and the oxygen gas was naturally electro-negative. Davy presumed that bodies only combined when they were in *opposite electrical states*. The copper, which was 'naturally positive', was brought 'artificially into a negative state' by contact with tin, or zinc, or iron. It was then protected by its artificial negative state from the negative oxygen. Presumably the chemical action proceeded to restore the disturbed electrical equilibrium by eating away the protecting metal, and enabling the artificially negative copper to drop into its customary positive condition.

Dr. Thomas Thomson, writing, just after Davy's death, in his *History of Chemistry*, 1830–1, thought that 'the electrical theory of affinity first proposed by Davy' had 'been universally adopted by chemists'. Davy identified the attractions between atoms with the electrical states of bodies. Thomson wrote 'atom', but Davy, who never believed in genuine indivisible atoms, would have written 'particle', or 'corpuscle'. Thomson illustrated Davy's notion by the combination between 'an atom of oxygen and an atom of hydrogen'—again writing 'atom' where Davy would have written 'corpuscle' or 'particle'. The oxygen atom had a 'coating of negative electricity'; the hydrogen atom was covered with positive electricity. The two atoms united because their opposite electricities attracted one another. The two atoms remained 'firmly united' because, since each atom attracted its own electricity, the 'respective electricities' could not leave their own atoms. If 'a current of the opposite electricities' passed through the compound it 'might neutralize the electricity in each' atom, and, 'thus putting an end to their attractions', cause decomposition. Such, in drastic summary, is Thomson's account 'of the electrical theory of affinity first proposed by Davy to account for the decompositions produced by electricity'. He himself described it as 'a very imperfect outline'.

There were enigmas then, as there are enigmas now, in electrical versions of chemical affinity. Davy's fundamental principle that electricity and

chemical affinity are closely related is still a principle of exposition. Exposition in 1929 is very different in detail from exposition in 1829, for ideas march far in a hundred years, but this fundamental notion persists.

VIII

DAVY AND DALTON

In 1826 a Royal Medal was awarded to Mr. John Dalton, F.R.S., 'for his development of the Theory of Definite Proportions, usually called The Atomic Theory of Chemistry'. Sir Humphry Davy, Bart., as President of the Royal Society, handed him the medal.

There was a great contrast between the two men. Davy became a Fellow of the Royal Society in 1803 when he was about 25. Dalton was 56 when the Royal Society made him a Fellow in 1822. Sir Humphry Davy had become President of the Society when he was about 42, two years before that. This striking difference was no casual consequence of incidental events. Admiring eyes did not watch Dalton as they watched Davy. The pontifical *Reviews*, the *Edinburgh* and the *Quarterly*, did not devote their pages to Dalton as they did to his brilliant contemporary. There was no popular fervour for Dalton as there was for Davy. The Royal Society simply repeated a constant contrast by honouring the younger man nineteen years before they honoured the elder.

The contrast ran through the characters of the two men, and through their reputations. Dalton never renounced his opposition to the Gay-Lussac Law of Volumes; Davy did renounce oxygen in chlorine. Dalton seldom altered an adopted opinion; Davy often did. A mental lissomness, that was not mere mental agility, distinguished Davy's genius from Dalton's talent. Dalton could track down an idea tenaciously, and grip it: it was well for chemistry that he could. His mind had not the range, nor the imaginative width, that Davy's had. He did not impress a sense of genius upon men as Davy did. Coleridge could not have said plausibly of Dalton, as he did of Davy, that he would have been foremost in poetry if he had not been foremost in science. The Dean of Ely observed no 'brilliant talents' in Dalton when he spoke from the President's chair to the British Association just after Dalton's death. He did perceive in him common sense, singularly clear understanding, slow deliberate approach, insistent thought, and genuine inductive methods. This estimate simultaneously reveals the qualities that did contribute a great concept to chemistry, perhaps the most determinative concept ever received by the science, and the failure to secure the brilliant position of Davy. Dalton had neither the wide mental range, nor the lissom brilliance, nor the quick imaginative insight, nor the experimental powers, nor the oratorical persuasiveness of his more immediately famous contemporary. The public eye fell on Davy

as naturally as it falls on a splendid light, but Dalton had no charm, nor immediate brilliance, nor eloquence to command such a fall.

The contrast ran again through their achievements. Davy contributed a long series of brilliant discoveries to science. The frequently dramatic quality of these discoveries was appropriate enough to the vivacious quality of his mind. Dalton contributed a single determining concept, or bunch of concepts. Though his name is associated with a few extra items, his Atomic Theory was his great contribution to science. It was a great, a very great, contribution. It imposed one of the great decisive influences, perhaps the most decisive influence of all, upon chemistry. The decisive strike of Dalton's Atomic Theory into research was the great event in chemistry, and most historians would probably say in the whole of science, during Davy's lifetime. The relations of Davy to the determining concept of his less brilliant contemporary, and to its consequences, constitute an important item in an historical account of his scientific work.

Previous to Dalton, and contemporary with him, chemists thought, or often thought, of very small indiscernible particles in substances. They might think, and often did, of these particles as *atoms*—indivisible and indestructible corpuscles. Dalton very definitely adopted *atoms* into his versions of phenomena. Before Dalton chemists had been very preoccupied with the *sizes* and *shapes* of the atoms or

corpuscles. He now threw the emphasis on the *relative weights* of atoms. Each element consisted of a set of precisely similar atoms, and each atom in each set had its own private relative atomic weight. The atoms of hydrogen, the lightest element, were assumed to be the lightest, and each atom of any other element was heavier than a hydrogen atom in a definite ratio. This notion was combined with another to produce some very decisive ideas.

It was natural, perhaps inevitable, to think of *flocks* of atoms or particles. Many small indiscernible corpuscles were the presumed internal mechanism of chemical combinations and decompositions. Dalton secured an important concept by singling out for separate contemplation *singly reactive atoms*.

Many chemists, however, thought as freely of the flock as of the single particle. If two substances, *A* and *B*, were combining, flocks of particles of *A* were crowding to unite with flocks of particles of *B*. Berthollet insisted on two factors that determined chemical reactions. The *affinities*, the attractions between the respective particles of *A* and *B*, were one determining factor. Great affinities promoted ready or vigorous action; small affinities promoted them less effectively. The composition of the final compound *AB* also varied with the proportionate presence of either *A* or *B*. Much *A* and little *B* tended to a compound with more of *A* and less of *B*, and vice versa. Much *A* had more particles to flock into combination, and little *A* had fewer. Berthollet

insisted that, in principle, any two elements or compounds, *A* and *B*, could unite in any proportion to form an indefinitely extensive series of compounds. In proportion as there was more of *A* or less of it there was more or less in the compound *AB*. Circumstances often intervened to prevent the formation of a compound containing a higher proportion of *A* or *B*, even if there were a large excess of either. Conditions often determined the actual production of one substance only, with a definite composition, however the proportions of the reacting substances varied. In principle, however, *A* and *B* could unite in any proportion according to their proportionate presence, according to the number of particles they flocked into the combination.

Berthollet was insisting on this principle in 1802 when the young Davy was lecturing at the Royal Institution. It seemed to Davy then that the 'number of different particles' in the 'sphere of activity' would influence combination. The 'force of the attraction of composition', he remarked, 'is influenced by the numbers of the combining particles'. Many particles with feeble affinities would be equivalent to a few particles with strong affinities. In 1806, when he was thinking of chemical affinity, or attraction, as 'electrical attracting energy', he thought that the effect of many particles, each with a 'feeble electrical energy', might equal, or be superior to, the 'effect of a few particles' each of which had 'a strong electrical energy'. He spoke of 'the influence of affinity by the

masses of the acting substances, as elucidated by the experiments of M. Berthollet'.

Meanwhile Dalton had been pondering over *gases*. A pile of shot represented for him the internal structure of, for example, oxygen gas. One atom of oxygen surrounded by an atmosphere of caloric was represented by each shot in the pile. Each atom was lodged, like a pea in the centre of a much larger spherical cushion, in a bloated globe of caloric. Each kernal *atom* had exactly the same weight, and each caloric atmosphere was weightless. Such caloric, or 'igneous', atmospheres were somewhat in vogue, and were to remain so for many years. Dalton assumed that in gases the caloric atmospheres were very much larger than the relatively tiny atoms. The constituent particles, the atoms with their caloric sheaths, were deduced from data: they were always, even in visible substances, too small to perceive. Hydrogen, or any other elementary gas, was analogously constituted. The caloric in the atmospheres was itself composed of very subtle particles.

The caloric atmospheres assisted to individualize the atoms, to direct an emphasis upon their singly reactive behaviour. When hydrogen and oxygen gases combined to form water Dalton assumed each hydrogen atom to unite with one atom of oxygen. The compound atom, as he usually called it, of steam, containing one atom of oxygen and one of hydrogen, was enveloped also in a caloric sheath, though the caloric atmospheres might dwindle, with

evolution of caloric, if the steam became water. The compound atoms of carbonic oxide (CO) analogously contained one atom of carbon and one of oxygen. The usual caloric atmospheres enveloped them also. The caloric sheaths protected the enveloped atoms from attack, but they did not give absolute protection. Second atoms of oxygen could, under favouring conditions, penetrate the protective caloric atmospheres of carbonic oxide. The resulting compound atoms of carbonic acid gas, enveloped in their turn by caloric atmospheres, each contained one atom of carbon and *two* atoms of oxygen. Carbonic oxide had a constant composition because its every compound atom contained one atom of carbon, with its own constant atomic weight, and one atom of oxygen with its own constant atomic weight. Since there were twice as many oxygen atoms in the compound atoms of carbonic acid gas this contained exactly *twice* as much oxygen in combination with the same amount of carbon as did carbonic oxide. In the opening years of the nineteenth century Dalton had surmised and disclosed similar *simple multiple relations* between different combining quantities in a handful of gases. They prevailed in the two oxides of carbon, in the oxides of nitrogen, and in the two gaseous combinations of carbon with hydrogen—olefiant gas (ethylene) and carburetted hydrogen (marsh gas). Berthollet was liberally prodigal with possible combinations, though circumstances often intervened to restrict their number. Dalton was

more restrictive, both in fact and in principle. Two atoms of one element might combine with three atoms of another into a single compound atom, and more complex combinations still might be possible, but the numbers of interacting atoms tended towards fewness. In any case the possible compositions were restricted by the conformity to the *simple multiple relations* imposed by the relative weights of the combining atoms. Dalton's principle can be thus stated. If the weight of each atom of hydrogen were taken as 1, then each atom of oxygen had a weight O, say, each atom of carbon a weight C, each atom of nitrogen a weight N, &c. These numbers, or 'proportions' as Davy usually called them, appeared in all combinations of these elements, either as such, or in simple multiples of them. Each element had, on Dalton's version, a definite atomic weight that represented the relative weight of its atoms, and appeared either as such or in a simple multiple, in all the combining proportions of the element. This notion was extended beyond the original handful of gases to which it was first applied.

Dr. Thomas Thomson received a personal exposition of the Atomic Theory from Dalton in 1804. He sketched the theory for the world in the third edition of his *System of Chemistry* in 1807. In the autumn of that year he failed to convince Davy, who dissuaded Davies Gilbert. Dr. Wollaston, however, converted Gilbert, and Gilbert then converted Davy. Thomson never knew what arguments he employed, but he

knew they were convincing, for 'Davy ever after became a strenuous supporter of the atomic theory'. Thomson wrote 'atomic theory', but, in his account in his *History of Chemistry*, 1830–1, he recorded Davy's significant 'alteration'. Davy substituted 'proportion for Dalton's word atom' to avoid 'theoretical annunciations'. Davy never believed in *atoms*, though he sought help from the 'proportions', and found it. The 'proportions' in which elements combined helped him out of his period of perplexity that ended with his disclosure of 'undecompounded chlorine'. They figured in his *Elements of Chemical Philosophy* in 1812 and in his subsequent writings. He never believed that the 'proportions' represented genuine atomic weights, nor that indivisible atoms existed. Very significantly Dalton received his medal for his Theory of Definite Proportions that was usually called The Atomic Theory of Chemistry. Sir Humphry no doubt helped to draft the title as well as hand the medal. He certainly did not direct his eulogium upon the *atoms*. 'Mr. Dalton' had, no doubt, developed 'the Newtonian doctrine of indivisible atoms', but he was the Kepler of chemistry because he had 'discovered a simple principle, universally applicable', of 'fixing the *proportions* in which bodies combine'. Wollaston's 'table of equivalents', Sir Humphry Davy significantly added, separated the 'practical part of the doctrine from the atomic or hypothetical part'. The 'atomic or *hypothetical* part': Davy never swerved from his distrust of it. In an

unfinished dialogue by Davy *Philochemicus* considered Dalton too much of an Atomic Philosopher, though the 'expression of the quantities in which bodies combine', the 'essential and truly useful part of his doctrine, was independent of such vain speculation'. *Vain speculation*: Davy's distrust of speculative atoms gathered rather than lost momentum. The *Unknown*, a participant in a dialogue composed by Davy shortly before his death, was probably, according to Dr. John Davy, the author 'as he wished to be'. The *Unknown* was certainly no friend to atomism when he identified 'the ultimate particles or atoms' with 'mere creatures of the imagination'.

The little solid Newtonian or Daltonian atom was at least triply menaced when it was urged on chemistry in the nineteenth century. When Dalton exclaimed that no man could split an atom he conflicted with one menacing difficulty. If an atom had parts, and it had them though they were very small, it should be divisible. This menace to the atom could be avoided by supposing that nature did not in fact divide what could in principle be divided. Sir Humphry Davy insisted neither on this objection to indivisible atoms nor on a criticism of the reply. The second menace to the Daltonian atom was its *hypothetical* nature. Atoms always seemed to Davy to be too hypothetical to be other than vain speculations.

The little solid Daltonian indestructible particle was also menaced by the Boscovich force-atom. In

1844, the year of Dalton's death and fifteen years after Davy died, Faraday identified atoms with spreads of force from centres. Such notions had been long prevalent in the tradition. In 1758 Boscovich had resolved atoms into points provided with attractive and repulsive forces. Dr. Joseph Priestley, 1733–1804, preferred a system of forces to impenetrably solid matter. Davy lingered over this idea. He protested in 1812, when Dalton's *New System of Chemical Philosophy*, 1808–10, had systematically urged atoms upon chemistry, that 'indivisible particles' need not be presumed in bodies: 'physical points endowed with attraction and repulsion' were as adequately expository as Dalton's atoms. The last paragraph that Davy wrote, in his *Consolations in Travel*, returned to this notion based on the 'hypothesis of Boscovitch', considering 'the elements' of bodies to be merely 'points possessing weight and attractive and repulsive powers'. Coleridge, who resented the 'corpuscular philosophy, as modified by the French theory of chemistry', credited Sir Humphry Davy with elevating 'the art of chemistry to the dignity of a science', and with inflicting a mortal blow upon the 'Corpuscular School' by promoting a 'dynamic spirit' in the 'physical sciences'. The little solid nucleus, the Newtonian or Daltonian atom, was constantly menaced by resolution into forces.

Dalton believed firmly in atoms, and Dr. Thomas Thomson preferred them. There was, however, a

very wide distrust of the genuine existence of atoms. Chemists usually presumed them, even if they did not finally believe in them, because they were so necessary for exposition. Davy never even adopted them for expositional convenience. He never used a chemical formula, nor a chemical equation. He did use exposition by particles, for it was difficult to discuss chemical reactions without ever speaking of particles or corpuscles. In 1802, at the beginning of his career, he evidently regarded even the interacting corpuscles as *methods of expression*, as a chemical language. He had always a lively sense of the *hypothetical* in science. Atoms were too hypothetical for credence. Corpuscles were hypothetical also, though they were too expositionally convenient to be banished from discussions of chemical phenomena.

In spite of his hostility to atoms Davy anticipated, in his own way, a future change of English opinion. Dalton assumed that *one* atom each of oxygen and hydrogen united into the compound atom of steam or water. Davy considered that 7·5 parts of oxygen by weight combined with 1 part of hydrogen. The *atomic weight* of oxygen was then 7·5 on Daltonian principles, because one atom of oxygen that was 7·5 times as heavy as a hydrogen atom invariably united with one atom of hydrogen. The *proportion* of oxygen was 7·5 on Davy's principles. The *Edinburgh Review* complained in 1811 that its favourite chemist persisted in speaking of *five proportions* of oxygen instead of 37·5 parts (5 × 7·5). Davy had adopted the

proportions, though he had no intention of adopting the atoms. The unfortunate *Edinburgh Review* had to reconcile itself to another change by its favourite chemist in 1812. He had doubled the proportion for oxygen to 15 in his *Elements of Chemical Philosophy*. This doubling, that Sir Humphry retained to the last, again contrasted him with Dalton. The proportion 7·5 corresponded to the HO version of water, that Dalton clung to, though he used his own cumbrous symbols; the proportion 15 corresponded to the H_2O version of water that most English chemists did not adopt for many years. Davy doubled to 15 because *two volumes* of hydrogen combined with *one volume* of oxygen. Berzelius, corresponding atoms to volumes, presumed H_2O. Davy, corresponding proportions to volumes, presumed 15 to be the proportion for oxygen. The fundamental ratio was the experimentally determined 7·5 parts of oxygen by weight (now 8 parts) that combined with 1 part of hydrogen. The atomic weight of oxygen was 7·5 if its single atom united with one of hydrogen. If its single atom united with two atoms of hydrogen its atomic weight must be 15 ($7·5 \times 2$) to correspond to the 2 parts of hydrogen. The HO version of water implied 7·5 (now 8) for the atomic weight of oxygen; the H_2O version implied 15 (now 16). Conversely, an atomic weight for oxygen of 7·5 implied HO, and 15 implied H_2O. Davy began with a proportion for oxygen that implied HO in atomic terms, and rapidly adopted a doubled number that implied

H_2O. He permanently adopted two *proportions* of hydrogen, each being 1, and one *proportion* of oxygen, that was 15, in water, just as Berzelius adopted two *atoms* of hydrogen and one *atom* of oxygen in the compound atom of water or steam. Davy had anticipated, in his own method of expression, the future English H_2O. He very frequently did anticipate alterations of versions, and he anticipated here.

IX

LAST DAYS

SIR HUMPHRY DAVY gave his last Bakerian Lecture, *On the Relations of Electrical and Chemical Changes*, on June 8, 1826. His health had begun to fail in 1823; after the death of his mother in the September of 1826 it steadily declined. He travelled painfully to Ravenna in the late January of 1827, and returned to England in the October of the same year. His resignation of the Presidency of the Royal Society, before his return, cast a shadow of the coming event. During this year, when he was seeking vainly for lost health, the fourth edition of his *Elements of Agricultural Chemistry* appeared.

It perpetuated another permanent reversal of his 'infant speculations'. He had committed his first youthful opinions to a choice between two competing traditions. Berthollet was insisting that physical and chemical forces were competent to produce

living things, and the whole phenomena of life. Chemical action might be brisker in animate beings, but no supplemental *vital force* was required. Berthollet represented the mechanistic or materialistic tradition that living things were simply more elaborate chemicals than minerals or other inanimate objects. As Berthollet knew well, it seemed to 'some celebrated physiologists' that, though animals and plants were chemical laboratories, there was an extra agency within them. The supplemental agency that operated in living things and distinguished them from inanimate beings received different names, as it received different speculative qualities, from different thinkers. It was often called the *vital force*, and chemists frequently preferred this term. The young Davy agreed with Berthollet and the mechanists that plants and animals were simply more elaborate chemicals than mineral objects. The 'simple laws of attraction and repulsion' were sufficient for the complex 'generation and growth of vegetables'. A plant only differed from a crystal by a greater complexity. An animal only differed from a vegetable by being still more complex. Life was a 'perpetual series of peculiar corpuscular changes', and perceptions or ideas, or pains, or pleasures were simply a set of consequences of this series. Light was a great promoter of the peculiar corpuscular changes that characterized living beings. Since material changes corresponded to changing sensations, light, their great promoter, was essential to 'perceptive exist-

ence'. The young Davy drew no atheistic inference from this mechanistic version of life. The 'great Organizer' presided over all; though law governed matter, it was 'impressed by the will of Deity'.

Sir Humphry Davy had very decidedly withdrawn this mechanistic version in 1812. Perfectly distinct laws then seemed to him to control dead and living matter: material powers were subservient to life, and material elements were the instruments of a 'superior principle'. He was still impressed in 1827 by the 'peculiar character' impressed by 'life' upon 'all its productions'. The 'power of attraction and repulsion, combination and decomposition' operated in the vegetable kingdom, but subserviently to life. Most chemists then agreed with Davy. The contrast between the mouldering corpse and the animated living body peremptorily demanded the concept of a *vital force*. Physical and chemical forces could not build the amazing structure of the animal, nor even the fabric of the plant. A directive *vital force* controlled the physical and chemical forces during life. Death displayed the consequence of subjection to physico-chemical agencies alone. Death, as Berzelius cried, delivered the dead body into the hands of purely chemical and physical powers. Decay and corruption then revealed that the fugitive *vital force* had directed and controlled the physico-chemical agencies during life.

Vegetable chemistry, however, deferred to 'the doctrine of definite proportions'. This remark is

a significant index to the then trend of thought. Extreme vitalistic versions had inclined to dispel even the fundamental laws of chemical combination from the world of life. A vitalistic fervour had suspected, and even affirmed, that there could be no chemistry of plants and animals. This error was vanishing, the fundamental laws of chemical combination were perceived to operate even where the vital force was presumed to subject physico-chemical agencies to its directive predominance, and Davy recognized that an *organic* chemistry, a chemistry of living things and their products, was possible.

The incessant transformations of the material world had constantly presented it as the perpetual battle-field of opposing powers. There would be 'a kind of eternal sleep in the physical world', Sir Humphry Davy urged on readers of his *Elements of Agricultural Chemistry*, 'if there were no opposing powers'. Gravitation was opposed by 'projectile motion, or the centrifugal force', or 'mechanical powers'. The 'repulsive energy of heat' opposed 'cohesion and chemical attraction'. Opposing powers battled together in 'the harmonious cycle of terrestrial changes'. By the 'attractive powers' of 'chemical elements', and 'by the influence of heat, light, and electrical powers, there is a constant series of changes; matter assumes new forms, the destruction of one order of beings tends to the conservation of another; solution and consolidation, decay and renovation are connected; and whilst the parts of the system

continue in a state of fluctuation and change, the order and harmony of the whole remains unalterable'. Vitalism, with its presumption of a *vital force*, tended to think of the living being as a battle-ground between physico-chemical agencies and the vitalistic disposing power. The vital force constantly built up the plant or animal; chemical and physical forces constantly tore at the cells and tissues. Before death ended the perpetual struggle, the vital principle, or force, constantly strove, with a successful constructive and maintaining effort, against the devouring physico-chemical agents. Then death snatched the fabric from the hand of life to deliver it into the hostile and destructive hands of purely physico-chemical powers. A vivid realization of this internal struggle characterized one phase of vitalistic thought that persisted till about the middle of the century. Davy probably thought more of a co-operative opposition than of insurgently hostile inorganic forces.

In his *Consolations in Travel*, a series of dialogues written shortly before his death, Davy made *The Stranger* speak of the 'shell-fish and coral insects of the *first creation*'. Davy had not withdrawn his vitalistic supplement to matter in the world of life: the first living things could not issue from a non-animate domain. In 1822, when he presented the Copley medal to Buckland, Davy had made a significant comment. Sound geology had dispersed 'the eternal order of one simple system', maintaining the

'same beings, slightly changed', destroyed by water, and renovated by fire. Geology had discerned a progressive development in the appearances of living beings upon the earth. It discerned 'organized germs passing, in the immensity of time, through the different stages of improvement, rising from fishes through mermaids, quadrupeds, and apes: and, at last, perfect in man'. *Mermaids!* Did Sir Humphry Davy joke, or did he seriously intend to include a tradition in a scientific generalization? Whether he joked or not, or merely meant the dugong, he had distinctly anticipated Darwin.

On March 29, 1828, Sir Humphry Davy left England again: this time never to return. Just before he began this last continental journey he communicated a paper *On the Phenomena of Volcanoes* to the Royal Society. It was read to the Society on March 20. His last paper to the Royal Society was read to it on November 20 of the same year. This paper, *An Account of Some Experiments on the Torpedo*, indicated that his habits of research were lasting to the end. This last research ended inconclusively with the surmise that 'animal electricity' was 'distinctive and peculiar'. Perhaps it was no inappropriate epitaph even for one who had ended so many researches so conclusively. Davy always realized that the achievements of the moment were steps to the achievements of the future. The concluding hope of the paper that his present 'imperfect trials' might lead to 'more extensive and profound researches' expressed

the continuous co-operation between the promoters of science. On May 28, 1829, accompanied by Lady Davy, he reached Geneva. Early in the morning of the next day, after his wife had told him of the death of Dr. Thomas Young, he himself ended his labours and his pains. The child who had been born at Penzance, nearly fifty-one years before, to enduring fame, was laid to his last rest at Geneva, in the cemetery of Plain-Palais.

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